

MIND
 A QUARTERLY REVIEW
 OF
 PSYCHOLOGY AND PHILOSOPHY

I.—WHITEHEAD AND PRINCIPIA
 MATHEMATICA.

BY BERTRAND RUSSELL.

THERE is in some quarters a tendency to suppose that Whitehead's part in our joint work was less than in fact it was. As no one except myself now knows what were our respective shares, I will try to state the facts as nearly as I can remember them.

Whitehead was already a lecturer at Trinity when I was a freshman; in my first term I attended his lectures on Statics. He was one of the examiners when I was a candidate first for a scholarship and then for a fellowship. He was always quite exceptionally kind, and passed by gradual stages from a teacher to a friend.

In 1900 we went together to the International Congress of Philosophy in Paris, where I was impressed by Peano. I saw that methods analogous to his would clarify the logic of relations, and I was led to the definitions of cardinal, ordinal, rational, and real numbers which are given in *The Principles of Mathematics*. Very soon Whitehead became interested. The project of deducing mathematics from logic appealed to him, and to my great joy he agreed to collaborate. I knew that my mathematical capacity was not equal to accomplishing this task unaided. Moreover, in June 1901 I came upon the contradiction about classes which are not members of themselves, and from that time on a large part of my time was occupied with attempts to avoid contradictions.

In the early part of the *Principia*, Whitehead contributed the treatment of apparent variables and the notation $(x). \phi x$.

Chapters 10, 11, 13 of the *Principia* are in the main his work.

He also invented the notations $D'R$, $\overrightarrow{R'x}$, $R''\alpha$ —in this last case, the concept as well as the notation. (My previous attempts at relational notation, which were clumsy, will be found in Peano's *Revue de Mathématiques*, Vols. VII and VIII.)

In the later parts, the primary responsibility was Whitehead's as regards cardinal arithmetic and mine as regards relation arithmetic. Whitehead alone was responsible for the section on Convergence and Limits of Functions, and for Part VI, on Quantity. Whitehead also contributed some portions which might have been thought to be more in my province, for instance the "Preparatory Statement of Symbolic Conventions" at the beginning of Vol. II, which is concerned with types and systematic ambiguity. He also wrote the bulk of the first chapter of the Introduction.

Whitehead was to have written a fourth volume, on geometry, which would have been entirely his work. A good deal of this was done, and I hope still exists. But his increasing interest in philosophy led him to think other work more important. He proposed to treat a space as the field of a single triadic, tetradic, or pentadic relation,¹ a treatment to which, he said, he had been led by reading Veblen.

When our work was sufficiently advanced, we parcelled out the topics, each produced a first draft of whatever was in his assignment, the other then went over it and probably made considerable changes, and then his revised draft was finally revised by the first author. In most parts of the book, there was, in the end, very little for which either had *sole* responsibility.

For the ten years from 1900 to 1910, the book took up practically all the available time of both of us. Our collaboration was always completely harmonious. Whitehead was more patient and accurate and careful than I was, and saved me often from a hasty and superficial treatment of difficulties that I found uninteresting. I, on the other hand, sometimes thought his treatment needlessly complicated, and found ways of simplifying his drafts. Neither of us alone could have written the book; even together, and with the alleviation brought by mutual discussion, the effort was so severe that at the end we both turned aside from mathematical logic with a kind of nausea. It was, I suppose, inevitable that we should turn aside in different directions, so that collaboration was no longer possible.

¹ And generally a space of n dimensions at the field of an $(n + 1)$ -adic relation.

II.—ALFRED NORTH WHITEHEAD (1861-1947).

By C. D. BROAD.

WHEN Alfred North Whitehead died on 30th December, 1947, in his eighty-seventh year, the English-speaking world lost one of its deepest and most constructive thinkers. The last twenty-three years of his life were spent in the United States at Harvard University and much of his most important work was done there. He had a deep affection and a great admiration for the country which had become his second fatherland. But he was also a typical product of Victorian England and Victorian Cambridge, and he never forgot the immense debt which he owed to his country, his public school, his university, and his college.

Whitehead was born on 15th February, 1861, at Ramsgate. His father, like W. E. Johnson's, inherited from his own father the headmastership of a private school. But he gave up the scholastic profession for clerical duty in 1867. From 1871 till his death in 1898 he was vicar of St. Peter's, a large parish with a church about three miles from Ramsgate. One of Whitehead's brothers entered the Church and became Bishop of Madras; another was for many years a schoolmaster at Sherborne. Mathematical ability would seem to be hereditary in the family; for Whitehead's nephew, son of the Bishop of Madras, is an eminent pure mathematician who at present holds the Waynflete Professorship in Oxford.

Whitehead was at school at Sherborne from 1875 to 1880. He was very happy there, and speaks of the excellence of the classical teaching in his time. In his last year he was head of the school and captain of games. He came up to Trinity College, Cambridge, in 1880, and he speaks with warm admiration and gratitude of the intellectual stimulus which he there received. Among his chief friends as an undergraduate were Henry Head, d'Arcy Thompson, James Stephen, the brothers Llewellyn Davis, Lowes Dickinson, Nathaniel Wedd, and Sorley. He was an active member of the *Apostles*, which at that time consisted of from eight to ten undergraduates or young B.A.'s, but was often graced by the presence of older members, such as Maitland, Sidgwick, Verrall, Henry Jackson, and week-end visitors who had left Cambridge for the wider world.

In 1885 Whitehead was elected to a fellowship at Trinity. In the same year he was appointed to a College lectureship in mathematics, and he held that office until 1910. He was married in 1890 to Evelyn Willoughby Wade. They had three children, two boys and a girl. Both boys served in the war of 1914-18, and one lost his life in his aeroplane in 1918.

Whitehead's life after taking his degree falls into three successive parts, spent in Cambridge, in London, and in the Cambridge across the Atlantic. It is also roughly true to say that his work falls into three divisions which correspond to these three periods in his life, though there is no breach of continuity but only a widening of his explicit interests and a gradual change of focus.

The move from Cambridge to London took place in the summer of 1910. For the first year Whitehead held no academic position; from 1911 to 1914 he held various posts at University College; and from 1914 to 1924 he was Professor of Applied Mathematics in the Imperial College of Science at South Kensington. His life in London was a very busy one, and it gave him many opportunities of seeing for himself how men act and react in co-operation and in conflict with their fellows. He was at one time or another Dean of the Faculty of Science, Chairman of the Academic Council, and an active member of many educational committees. There is no doubt that he found his life in London intensely stimulating and that it brought out much in him which had hitherto been latent. He began working on explicitly philosophical problems towards the end of the first world-war, and took an active part in the discussions of the Aristotelian Society.

In 1924 Whitehead was paid the high compliment of being invited to join the philosophical department at Harvard. It has been given to few men to start a new career in a new country at the age of 63 and thereafter to spend at least twenty more years in extremely original intellectual activity of the highest quality along quite fresh lines. Whitehead's output during this last period would be astonishing even in a man still in the prime of life. He and Mrs. Whitehead found themselves completely at home in their new surroundings, and made themselves greatly beloved by American colleagues and students and by visitors from England, to whom they showed all the old kindness and hospitality which had been characteristic of them in Cambridge and in London.

In 1937 Whitehead became Professor Emeritus at Harvard, and in 1945 England bestowed on him the highest honour at

her disposal, the Order of Merit. He had become a Fellow of the Royal Society in 1903, and in 1931 he had the almost unique distinction of combining this with a Fellowship of the British Academy. (I believe that Sir J. G. Frazer received this double honour, and I do not think that anyone else has done so.)

It is now time to say something about Whitehead's contributions to learning. It may fairly be said that he began as a mathematician with strong philosophical interests; made very important contributions to the philosophy of physics in his middle period; and ended as a constructive metaphysician, with a mathematical and physical background, who endeavoured to synthesise every important aspect of reality in one all-embracing system. A very important fact about Whitehead was that at no period of his activity was he 'philosophising on an empty stomach', if I may use that expression. Apart from his technical mastery of mathematics and symbolic logic, he had a wide and deep knowledge of history in general and the history of natural science in particular; he had had much practical experience of men and institutions, though it was inevitably confined within a somewhat narrow range; he was steeped in the best European literature and he had a sympathetic understanding of certain forms of religion; and he had an exceptionally sane and balanced personality, free from crotchets and grievances. His work, therefore, never gives that impression of thinness and clever-silliness which sometimes characterises the productions of highly intelligent writers who have lacked these advantages.

The chief work of his earliest period is his *Universal Algebra*. This begins with a general theory of the subject, and then proceeds to an elaborate and original development of the Algebra of Logic and of the Extensional Calculus of that great but neglected genius Grassmann. A second volume was projected, and Whitehead worked at it for several years. But he became absorbed in his collaboration with Russell in *Principia Mathematica*, and he may have thought that the latter work would eventually embody in an improved and generalised form all that he had intended to put into this second volume. It should be remembered that *Principia Mathematica* was to have been completed by a volume on geometry by Whitehead, which never appeared.

Mention must here be made of the difficult, but brilliantly original and fundamental paper, entitled *Mathematical Concepts of a Material World*, which Whitehead contributed to the *Philosophical Transactions* of the Royal Society in 1905. To quote his own words: "The memoir is concerned with the

possible relations to space of the ultimate entities which (in ordinary language) constitute the 'stuff' in space". Whitehead formulates and works out in detail five alternative 'Concepts'. One of them is a formal statement of the presuppositions of classical physics and geometry; some of the others involve very radical departures from that scheme of thought.

To this period belong also tracts on *The Axioms of Projective Geometry* and *The Axioms of Descriptive Geometry*, and the admirable popular book on *Mathematics* which he contributed to the Home University Library.

In the second period Whitehead devoted himself to the philosophy of mathematical physics, at the time when the theory of relativity had stirred the waters to their depths and slightly before the impact of the quantum theory had been generally felt. In the *Principles of Natural Knowledge* and the *Concept of Nature* Whitehead was mainly concerned with the following three problems. (1) To overcome the familiar dualism between a world of scientific objects, supposed to be knowable only as remote causes of sensations, and a world of sense-data, supposed to be private and mind-dependent. (2) To show in detail, by means of the Principle of Extensive Abstraction, the connexion between the crude data of sense-perception and the refined mathematical concepts of point, instant, particle, instantaneous velocity, etc. (3) To deduce the transformation-equations of the special theory of relativity from extremely general considerations about our spatio-temporal experience, without reference to such concrete and contingent matters as synchronisation of clocks by means of light-signals.

In the *Principle of Relativity* Whitehead dealt in his own way with the general theory of relativity. The book (which, so far as I know, 'fell still-born from the press') divides into three parts. The first is mainly philosophical or epistemological; the second consists of a suggested new law of gravitation, different both from Newton's and Einstein's, and the deduction from it of certain results in astronomy, electricity, magnetism, and heat, by which it might some day be experimentally tested; and the third is a purely mathematical treatment of the general theory of tensors, which approaches the subject in a purely abstract way and does not put geometrical interpretations on the various special tensors which are introduced.

It may be of interest to enumerate the points of agreement and the points of difference between Whitehead and orthodox relativists like Einstein. The points of agreement are the following. (i) The fundamental relations in nature are not

spatial or temporal but are spatio-temporal. (ii) The fundamental stuff of nature is not things, but 'events' which have duration and spatial extension. (iii) There is not just one unique way of slicing up space-time, leading to one unique space and one unique time. There is an infinite plurality of different 'directions' in space-time, each of which is equally permissible as a time-axis for dating all physical events. Corresponding to each of these there is a certain timeless space; and all these timeless spaces are equally permissible for locating all physical events, provided that the corresponding 'direction' in space-time is used as the time-axis for dating them. (iv) Not all 'directions' in space-time are permissible as time-axes. All those which are so are confined within a certain four-dimensional cone. (v) The fact that the fundamental relations in nature are spatio-temporal necessitates a modification in the form of the traditional law of gravitation. That law takes account only of instantaneous distances between *bodies*, whereas a genuine law of nature must be in terms of *spatio-temporal* separation between *events*. (vi) The laws of nature must be expressible in tensor form. (vii) Einstein's law of gravitation, when properly interpreted, is at least a *possible* form of the law.

The following are the main points of difference. (i) As already noted, Whitehead reaches the transformation-equations of the special theory of relativity from very general considerations and without any reference to experimental facts about the velocity of light and the synchronisation of clocks by light-signals. Among other advantages of his method he claims that it enables him to define parallelism and normality, to give a clear meaning to the notion of a Newtonian frame of reference, and to solve the old philosophical difficulties about absolute rotation. (ii) In opposition to the orthodox relativists, who hold that the structure of space-time is not uniform always and everywhere but varies with its 'contents', Whitehead asserts and claims to prove that space-time is and must be homaloidal in structure. (iii) Nevertheless the traditional law of gravitation can and must be modified, for the reasons stated above.

Whitehead claimed to prove that space-time must be homaloidal by two arguments, one epistemological, and the other based on certain empirical facts about time-measurements. I find both arguments most obscure and unconvincing, as stated.

It is rather depressing to consider how little attention has been paid by professional theoretical physicists and pure mathematicians to the many highly original ideas which Whitehead put forward and often worked out in considerable technical

detail. They alone are competent to pass a final judgment on the validity and importance of these ideas, and they cannot excuse themselves (as in many cases they quite justifiably might do) by saying that it is the work of a cranky amateur of doubtful competence. I have spoken to many of my mathematical and physical colleagues, who are intensely interested in these subjects, and I do not think that I have ever found a single one of them who had troubled to read what Whitehead had to say. I must admit, however, that the fault was largely that of Whitehead himself. He was an abominably obscure and careless writer, and this fault certainly grew on him as he became older. It is the more deplorable, since he certainly possessed at one time the power to write clearly, as his little book on *Mathematics* shows, and since he certainly retained that power when he chose to exercise it, as is shown by many passages in such later works as *Science and the Modern World* and *Adventures of Ideas*.

I shall touch very briefly on the work of Whitehead's third period, in which he developed what has been called 'the Philosophy of Organism'; partly because I understand it so imperfectly, and partly because it is to be treated by another contributor who has made a special study of it. The central work of this period is the Gifford Lectures, *Process and Reality*, but this should be taken together with its predecessor, *Science and the Modern World*, and its successor, *Adventures of Ideas*. *Process and Reality* is one of the most difficult philosophical books that exist; it can vie in this respect with the works of Plotinus and of Hegel. I cannot pretend to understand much of it, and I cannot help thinking that many of its enthusiastic admirers must simply be counted among those who "wonder with a foolish face of praise". It is often desperately difficult to understand what it is that Whitehead is asserting. When one is fairly sure of this, it is often equally hard to discover what he considers to be the reason for asserting it; for he seems often to be "not *arguing* but just *telling you*". And, finally, when one thinks that one knows what he is asserting and what he is alleging as the ground for it, one often fails to see how the latter proves or makes probable the former. *E.g.*, I am fairly certain that Whitehead thought that he had given an answer in his later books to the difficulties which Hume raised about the validity of induction. I am also fairly certain that he thought that his own account of sense-perception and of the way in which we come to believe that this causes that (which are enormously better than anything that can be found in Hume) furnish a satisfactory basis for an answer to these difficulties.

But I cannot for the life of me understand how precisely they are supposed by Whitehead to do so.

Still, from my knowledge of Whitehead and of those of his writings which I think I can understand, and from the occasional gleams and glimpses which have been vouchsafed to me in struggling with *Process and Reality*, I feel fairly certain that there is something important concealed beneath the portentous verbiage of the Gifford Lectures. Whitehead often makes profound and thought-provoking observations, expressed in happy and striking phraseology, and exhibiting real wisdom as distinct from mere cleverness. These are particularly prominent in *Science and the Modern World* and *Adventure of Ideas*, which contain much that is perfectly intelligible and intensely stimulating to any intelligent reader.

It is, perhaps, fortunate for most prophets that, unlike Whitehead, they have been without honour in their own countries and times. A lesser man than he might easily have been spoiled by the adulation which his later work received and by being treated as a kind of Messiah by many of his more foolish admirers. Nothing of the kind happened. He remained simple, natural, modest, humorous, and intensely human. He was equally great in intellect and in character, and one of the finest products of that very fine civilisation and culture which have perished in England's two Pyrrhic victories over Germany.

III.—PROFESSOR H. A. PRICHARD. PERSONAL RECOLLECTIONS.

By E. F. CARRITT.

I FIRST knew Prichard in 1896 when I began to read for Greats at Hertford College and, to my lasting good fortune, found him, some four years older than myself, my tutor in philosophy. Since then, with intervals in times of war, I suppose I have talked philosophy with him nearly once in every week of terms and frequently corresponded on points raised either by him or by myself. Our last long discussion of this kind was in the autumn of 1947, when his criticism, and not least his self-criticism, was as acute and fundamental as ever.

I can hardly imagine the type of pupil for whom he would not have been an ideal tutor. I cannot say how much I myself owed to him in that capacity or later as a friend and critic. I had been interested in moral philosophy and æsthetics as a schoolboy, but, having done poorly in classical moderations, came to Greats in a rather disgruntled mood which, under less sympathetic and stimulating treatment, might have developed into recalcitrance or dilettantism. The first was made impossible by Prichard's friendliness, and the second intolerable by his trenchant criticism. His sincerity and enthusiasm were infectious.

As reported in legend his bubble-pricking may sound too drastic to have been acceptable, but as actually experienced in this atmosphere of equality and fraternity it was bracing. There is, for instance, the story of his receiving some dictum with the remark : "Well, whatever may be true, that can't be". But it has also to be recorded that this, in fact, was to a colleague rather senior to himself and was immediately followed by a grin and the apology : "Though not quite in that tone of voice". Yet, when the opponent repeated the view in rather richer language, the no less devastating rejoinder was ready : "Aren't you just wrapping it up in cotton-wool ?" And always one felt that he was not contending for victory but co-operating in a pursuit so serious and eager that rivalry and ceremony were forgotten. His most frequent criticism was : "You seem to be letting yourself off rather easily". And one of the highest praises I have known him give to a contemporary philosophical

writer was : " I don't agree, but he is always worth reading ". But then one knew that he would have felt that to be high praise for himself. Certainly letting himself off easily was a censure he never incurred.

Probably his impatience and acute criticism of loose or vague statement were partly due to his mathematical training (he had taken a first in mathematical moderations) ; and to those with more Horatian maxims of language it sometimes seemed that he treated words like mathematical symbols independent alike of common usage and of context. On the other hand I have heard him say that of all the school discipline he received at Clifton the most profitable was the drill in Greek conditional sentences.

In the 1890s Oxford philosophy was still mostly in the tradition of T. H. Green, Wallace and Bosanquet, the first two being recently dead and the last having left Oxford. This tradition was carried on by Edward Caird as Master of Balliol and by Blunt of Christ Church. Bradley was residing in Merton but was an almost legendary recluse, for, so far as I know, he never lectured and I never saw him, and I once heard Professor Cook Wilson say they had never met. In this tradition Prichard had been brought up and he was just beginning to break away from it under the stimulus of Wilson, whose lectures on Plato's Republic as an Oriel tutor and later on logic as Professor were the greatest living influence in the school at that time. Wilson started an informal weekly meeting of some dozen philosophy tutors for tea and the discussion of short papers, with a dinner and longer paper once a term. I was elected to this soon after I began to teach and well remember how the younger man would develop, from some criticisms of orthodox idealism by the elder, a conclusion not immediately welcome to their author. In particular I think Wilson, whose primary interest was logical, did not always see (as Prichard did) how this change of his must eventually affect his moral philosophy also ; and this may have helped to determine the latter's main concern to ethics. His only published volume is on *Kant's Theory of Knowledge*, of which some of us can say that it was the first on the subject which we ever understood. But more widely influential have been his article in MIND (1912), *Does Moral Philosophy rest on a Mistake?* ; his inaugural lecture as White's Professor (1928) on *Duty and Interest* ; his British Academy lecture (1932) on *Duty and Ignorance of Fact* ; and his article in *Philosophy* (1935) on *The Meaning of ἀγαθόν in the Nicomachean Ethics*.

From 1898 to 1928 as Fellow of Trinity and from then till 1937, when he retired under the age limit, as White's Professor and consequently Fellow of Corpus, Prichard was one of the moving spirits in the Oxford Philosophical Society (graduate) and the Jowett Society (undergraduate) as well as in the London Aristotelian and in the weekly tea I have mentioned. In the last, among the other protagonists of the earlier period were Professors J. A. Smith, Joachim and Webb, and H. W. B. Joseph, of whom Webb alone has survived, though Joseph lived till after his retirement. Among these six, discussion, with every variety of alliance and opposition, was always lively and often brilliant. Rather later, Ross, soon to become Provost of Oriel, was a sympathetic critic always ready to acknowledge his debts.

Even after his retirement, in spite of his devotion to his own studies and of a growing deafness, Prichard was assiduous in his aid to the Philosophical and Jowett societies. There could have been no greater tribute from the affection and admiration which his single-minded enthusiasm inspired than the congregation at his memorial service in Trinity College. As an old friend of mine, who was also his pupil at Hertford, but who had not seen him since, has written to me since his death: "My remembrance of his vivid blue gaze easily jumps over fifty years —and what years!"

IV.—BOOLE AND THE REVIVAL OF LOGIC.

BY WILLIAM KNEALE.

At the end of 1847 there appeared a pamphlet called *The Mathematical Analysis of Logic* which is considered by many mathematical logicians to be the beginning of their study. Mr. Alonzo Church, for example, has listed it first among a small number of works which contain new ideas of fundamental importance for the development of the subject.¹ In this paper, written to celebrate the centenary of the pamphlet,² I propose to give (1) a short account of the author, (2) an outline sketch of his work, and (3) a very brief estimate of the importance of his contribution to logic.³

(1)

George Boole was born at Lincoln on the 2nd of November, 1815. His father was a shoemaker with a liking for study which led him to take an active part in the local Mechanics' Institute. After attending a primary school of the National Society the young Boole went for a time to a school for commercial subjects. But he wished also to learn Latin, and for this purpose he took lessons from a neighbouring bookseller.

¹ In the bibliography of symbolic logic published as part of the *Journal of Symbolic Logic* for 1936.

² The substance of the paper was given as a lecture for the Moral Sciences Faculty in the University of Cambridge on 13th November, 1947.

³ The chief sources for his biography are an article by the Rev. R. Harley in the *British Quarterly Review* for 1866 and four articles by his wife on "The Home Side of a Scientific Mind" in the *University Magazine* for 1878. The memoir in the *Proceedings of the Royal Society* for 1867 and the articles in the *Dictionary of National Biography* and the *Encyclopaedia Britannica* add little, but there is a pleasing engraved portrait with the obituary notice in the *Illustrated London News* for 21st January, 1865. Venn's *Symbolic Logic* (1881) is in effect a commentary on his logical work, and is still useful. A more recent account of his contribution to logic is to be found in C. I. Lewis's *Survey of Symbolic Logic* (1918). The *Laws of Thought* was reprinted in 1916 as the second volume of *George Boole's Collected Logical Works* under the editorship of P. E. B. Jourdain; the first volume, which should have contained the other logical writings, together with a biography of Boole and an account of his work, was never published, but some results of Jourdain's historical researches about Boole are contained in an article contributed to the *Quarterly Journal of Mathematics* in 1910.

Having mastered the Latin grammar with the help of his friend, he went on to teach himself Greek from borrowed books. He was, it seems, a precociously learned child, but at this time his ambitions were all linguistic. His first printed work was a verse translation from the Greek of Meleager, which his father sent to a local newspaper with a note stating that the author was fourteen years of age. This gave rise to some controversy, because a schoolmaster who read it declared that it could not have been produced by a boy of that age. When he was sixteen, Boole became an usher in a school at Doncaster ; after a short time he returned to Lincoln, but he continued to teach children, either there or in the neighbouring village of Waddington, for eighteen years. When he was only twenty years of age, he opened a school of his own. It is said that he was a good teacher, and it seems that he must also have been reasonably successful as a business man ; for he was able to support his parents as well as himself on the proceeds of his enterprise, and was soon highly respected in the town as a solid citizen.

During his years of school teaching Boole continued his own education. French, German and Italian he added to his store as a matter of course ; and for two years he studied the works of the fathers of the church with the intention (later abandoned) of preparing himself for ordination. But the most important happening of this period was his discovery of higher mathematics at the age of seventeen. He had already acquired some interest in mechanics and optics from his father ; now he began to read for himself the works of the great mathematicians, although it was not until he had established his own school that he made mathematics his principal study. In later life he said that he took to this science because he no longer had access to a good library and found that, for a poor student who had to purchase his own books, mathematical works, which took a long time in the reading, were the best investment. He sometimes added that he had lost five years by his imperfect methods of study and his lack of expert guidance ; but it is arguable at least that he gained more than he lost, for the teaching of a grammar school and a university might perhaps have stifled his originality.

Very soon after he had begun to study mathematics seriously he had the idea that algebraic formulæ might be used to express logical relations. In later years he told his wife that the thought came to him during a walk in the fields while he was still a boy. The circumstantial detail in this story suggests that he had a vivid experience, something like that of Descartes by the stove at Ulm, and saw in a flash "the principles of a marvellous new

science". However that may be, his first published work on mathematics was a paper on the Theory of Analytical Transformations which appeared in the *Cambridge Mathematical Journal* in 1840. The editor of this recently established periodical was a young Fellow of Trinity called Gregory who saw the merit in Boole's early papers in spite of certain obscurities in presentation and helped him in preparing them for publication. This friendship continued until Gregory's death in 1844. That Boole valued it very highly is evident from a reference in a paper of that year¹ and from what we know of his conversation at a much later date.

About this time it was suggested to Boole by some friends that he should go to Cambridge in the hope of winning a fellowship there. Boole consulted Gregory, who set out all the relevant considerations very fairly. In particular, he pointed out that Boole would have to give up his freedom of study for some years and undergo a good deal of uncongenial mental discipline. "A high degree here", he wrote, "is due quite as much to diligent labour in certain appointed paths as to mathematical capacity". In the end Boole decided that he could not afford to try his future at Cambridge, since he now had the responsibility for supporting his two aged parents.² In later life he did not regret his decision to stay in Lincoln and study by himself. On the contrary, he often spoke of the unsatisfactory state into which the study of mathematics had fallen at the universities, and especially at Cambridge, where he said it was too little a training for the mind and too much a display of *tours de force*. According to Mrs. Boole, he maintained that a real mathematician must be something of a poet, and thought that a mathematical school of a higher order might grow up at Oxford. Why he should have made this flattering prediction about my own university I do not know; but I think it is possible to discover from his criticisms of current teaching what it was in mathematics that attracted Boole, namely, the beauty of generality. On another occasion he said "However *correct* a mathematical theorem may appear to be, you ought never to be satisfied there is not something imperfect about it till it gives you the impression of being also *beautiful*". Towards mathematical truth he had indeed a consciously religious attitude, which he sometimes expressed to

¹ *A General Method in Analysis*, mentioned below.

² It is surprising to find that according to Gregory an undergraduate living modestly at Trinity a century ago had to spend more than £200 a year, although the pound was presumably worth much more then than it is now.

himself by the phrase "For ever, O Lord, Thy word is settled in heaven".

The work which first established Boole's reputation was his essay on *A General Method in Analysis*. Just before his death Gregory advised that it should be submitted to the Royal Society and offered to arrange this, if Boole could not find a Fellow to submit it for him. It was printed in the *Philosophical Transactions* for 1844, and won the Society's gold medal as the most important paper of the past three years. At this stage in his development Boole was much interested in the generalisation of algebraic reasoning, and made free use of the calculus of operators (or the separation of symbols, as it was then called), but he had not yet applied his newly acquired skill to the problem of logic. In the spring of 1847, however, his attention was attracted once more to those speculations about algebra and logic which had occurred to him first while he was still a boy, and within a few weeks he had written the *Mathematical Analysis of Logic*. A first draft was sent to the Rev. Charles Graves, Professor of Mathematics at Trinity College, Dublin, who approved of it and suggested some ingenious additions. The work was then published as a pamphlet by Macmillan, Barclay and Macmillan of Cambridge with a preface dated the 29th of October, 1847. According to one story, it appeared in the shops on the same day as De Morgan's *Formal Logic*. This, if true, was a curious coincidence; for De Morgan was another mathematician who helped to produce a revival in logical studies, and Boole's renewed interest in logic was due to the appearance in periodicals of letters on the once famous dispute between De Morgan and Sir William Hamilton.¹

Copies of Boole's pamphlet are now scarce, and it cannot be supposed that many were sold. In the following year, however, he published a short account of his Calculus of Logic in the *Cambridge and Dublin Mathematical Journal*, and this may have been more widely read among mathematicians. It adds nothing to the theory of the pamphlet, but it is somewhat easier to follow, and it ends with a paragraph which summarises Boole's position very clearly: "The view which these enquiries present of the

¹ Hamilton (not the inventor of quaternions, but a pedantic Scottish baronet who held the chair of Logic and Metaphysics at Edinburgh) had foolishly accused De Morgan of adopting his doctrine of the quantification of the predicate without proper acknowledgments, and had added some stupid remarks against mathematics as an element in education. He was very properly ridiculed by De Morgan. There is an account of the controversy in an appendix to the *Formal Logic*.

nature of language is a very interesting one. They exhibit it not as a mere collection of signs, but as a system of expressions, the elements of which are subject to the laws of the thought which they represent. That these laws are as rigorously mathematical as the laws which govern the purely quantitative conceptions of space and time, of number and magnitude, is a conclusion which I do not hesitate to submit to the exactest scrutiny."

In 1849 Boole was appointed Professor of Mathematics in Queen's College, Cork, then newly founded. This was a turning point of his fortunes. Hitherto he had been handicapped in his studies, first by poverty and then by lack of leisure; now he had an academic position which gave him a salary adequate to his needs and time for research. To have gained this at the age of thirty-four was all the worldly success that a man of Boole's temperament required. In a recent book on the lives and works of eminent mathematicians there is a chapter which suggests that he was constantly moved by a desire to thrust his way into a higher social class.¹ It is said, for example, that he studied the classics as a boy because he thought they were the key to dominant living, and that he began to prepare himself for ordination because he saw this as an avenue to the status of gentleman. Boole had, indeed, a desire for fame—at least in the early part of his life—and he might, no doubt, have become a snob, if he had thought much about social distinctions; but for any one who takes the trouble to search there is plenty of evidence to show that he was not at all sensitive about his origin.

He was at all times acutely conscious of social evils, but his interest was that of a philanthropist, not that of a man who is sorry for himself. In Lincoln he was an unpaid teacher of classics and mathematics at the Mechanics' Institute, a trustee of the Female Penitents' Home, and a Vice-President of the Early Closing Association, which worked for a limitation of the hours of labour. When the members of the last-named association held a public meeting in 1847 to celebrate the introduction of the ten-hour day, it was Boole they invited to make a speech. His address on *The Right Use of Leisure*, which was printed at the request of the Association, is full of good sense. The reading lists which he supplied for various subjects are long since out of date, but his general sentiments are all that a modern supporter of adult education could wish. It is interesting to note in passing that the section on moral philosophy contains a warning against

¹ E. T. Bell, *Men of Mathematics*. There are also several inaccuracies in the account of Boole's life, but the estimate of his place in the history of mathematics is just and well expressed.

the "lax and defective" system of Paley, *i.e.* religious egoism. When Boole left Lincoln in 1849 to take up his professorship, the citizens organised a public supper in his honour, and the mayor presided. When he died fifteen years later, they put up a stained glass window in the Cathedral to his memory. I do not suppose this happens to many mathematicians, and I think it very unlikely that it would happen to any mathematician who was also a snob.

When he was settled in Cork, Boole began the preparation of a book about his logical theory. Of this period of his life he said that mental science was his study and mathematics his recreation. This does not mean that he was unproductive in mathematics (he composed notes on quaternions at this time), but that he spent a great deal of energy in familiarising himself with what had been written by philosophers about the foundations of logic. The result of his labour was *An Investigation of the Laws of Thought on which are founded The Mathematical Theories of Logic and Probabilities*. This work, which was published in 1854 at the expense of Boole and a friend, did not sell very well, but it has been read by many who have never seen the *Mathematical Analysis of Logic*, and it is often regarded as Boole's masterpiece. The chief novelty of the book is its application of his ideas to the calculus of probabilities. This is well done; but for the rest the book is not very original. There is no important change on the formal side, and it cannot be said that the main ideas are presented more lucidly than in the earlier work. There are, however, some elaborate examples and some metaphysical passages which are interesting, if only for the light they throw on Boole's reading.

During his professorship Boole received various academic honours. In 1852 he was made LL.D. of the University of Dublin, in 1857 Fellow of the Royal Society, in 1858 Keith prizeman of the Royal Society of Edinburgh, and in 1859 D.C.L. of Oxford. It is said that his appearance at Encaenia to receive the last-named degree passed almost unnoticed, because the undergraduates and the general public were full of enthusiasm for Sir John Lawrence and two other heroes of the recent fighting in India who received honorary degrees on the same day. To this part of his life belong various papers on probability and two books which were for a time standard texts, namely, his *Differential Equations* of 1859 and his *Finite Differences* of 1860. The material which he had collected for a second edition of the former was published after his death by Isaac Todhunter.

In 1855 Boole married Miss Mary Everest, a young lady of

little more than half his age who was the daughter of an eccentric Gloucestershire clergyman and a niece of the professor of Greek at Cork. According to the historian of mathematics mentioned above, he took this step because he was subconsciously striving for respectability through an alliance with the classics. It would be at least as plausible, I think, to suggest that he was striving for eminence through an alliance with Mount Everest, which was called after another of the lady's uncles. However that may be, Mrs. Boole for her part was very well aware of her husband's intellectual distinction and anxious to help him in his work, not only by organising the details of his life (which she did with a high hand), but also by reading his drafts and pointing out what seemed obscure. On one occasion Boole discovered to his great distress that she had told their five daughters that he was a genius as great as Dickens. He pointed out that, apart from other considerations, such talk was unwise because it would make the children expect special consideration from society, whereas they should be prepared to earn their living in an equalitarian world. Many years after Boole's death Mrs. Boole came to think of herself as the high priestess of a new revelation, but before this delusion had warped her judgment she produced a series of four simple and entirely convincing articles about the character and opinions of Boole during their married life. These contain much interesting information which could scarcely have been collected by anyone else.¹

In Ireland, where class distinctions were strengthened by racial and religious hostility, Boole's indifference to social rank aroused comment, and even Mrs. Boole was sometimes embarrassed

¹ See the note on sources at the beginning of this paper. The books which she produced from 1883 onwards were based on the belief that Boole's writings contained a religious and educational message which had been missed by the mathematicians and philosophers. There are more than a dozen of them with such queer titles as *The Message of Psychic Science to Mothers and Nurses*, *Logic Taught by Love*, *Suggestions for Increasing Ethical Stability*. The following passage (p. 29), taken at random from *The Philosophy and Fun of Algebra* (1909) will serve as a specimen of her later style: "The first Hebrew algebra is called Mosaism, from the name of Moses the Liberator, who was its great incarnation or Singular Solution. It ought hardly to be called an algebra: it is the master key of all algebras, the great central direction for all who wish to learn how to get into right relations to the Unknown, so that they can make algebras for themselves." She died in 1916 at the age of 84, and a little book in her honour was published as late as 1923 under the title *A Teacher of Brain Liberation*. This was by a devoted disciple, Florence Daniels. It contains one gem: "Like the disciple of Pythagoras, for two years I sat at her feet without speaking (this is metaphorical, of course)."

by the people he brought home. If he met anyone in a train or a shop whose conversation seemed to show an intelligent interest in nature, he would invite him to their house at Ballintemple near Cork to look through the telescope and talk science. As at Lincoln he had helped in the work of the Mechanics' Institute, so here Boole tried to do something useful for the chief local organisation of young men. This made a parade of virtue and culture in its title of "Temperance Band", but the Protestant gentry maintained that it was a really Fenian society. Whatever the dominant passion of the members may have been, they were apparently not prepared to learn mathematics. But they came to look through the professor's telescope from time to time, and were so evidently impressed by his good will that Mrs. Boole had no doubt her household would be safe in any Irish revolution. In general Boole's reputation among the poorer people of the neighbourhood was that of an innocent who should not be cheated. Among the higher classes he was admired as something of a saint, but thought to be rather odd.

Although Boole liked his fellow men and was welcome on social occasions of all sorts, from picnics to meetings of learned societies, there was one subject about which he was extremely reserved in public—religion. Mrs. Boole states that he wore a polite mask in the presence of people who were professedly religious, and persuaded her to do the same. The reason was not that he lacked interest in the matter. On the contrary, he spent a great deal of time reading theological and devotional literature of the most varied kinds from Aristotle to Böhme. Among the contemporary writers in this field those most to his taste were Frederick Maurice, the Christian Socialist leader, and Francis Newman, the Cardinal's brother, who ended in Unitarianism. For Maurice his admiration amounted almost to hero-worship. On one occasion, for example, he said "As long as Maurice lives, I will never write a word against an opinion which he holds", and at another time, shortly before his death, "I feel that I should not like to leave the Church while Maurice is in it". The reason for his avoidance of religious discussions was his profound dislike for the way in which these were commonly conducted. He attached the utmost importance to liberty of thought and insisted for this reason that his children should be kept away from clergymen or any others who would teach them there was *merit* in holding one set of opinions rather than another. In the privacy of his home he went so far as to say that fixed religious beliefs demoralised most people, citing it as a sad effect of dogma that even Maurice was unfair to Renan; and Mrs.

Boole tells us that "the things which commonplace, easy-going clergymen took it upon themselves to say of Bishop Colenso seemed almost to put him beside himself". When she asked him once to explain the meaning of some controversial writing which she had found obscure, he replied "Meaning? You mustn't suppose people *mean* anything when they talk in that way". Towards the end of his life Boole came to think that he should have been bolder in giving public support to the liberal cause in theology, but it was probably a sound instinct which had made him say in earlier years "It would never do for us to be mixed up with that set".

Our records are too scrappy for a full biography, but it is evident that Boole was a man of more than usual sensitivity. His wife declares that his horror of evil was sometimes so intense as to produce a morbid depression, and says that at such times she thought it her duty to make fun of him. Apparently he took this in good part. He was particularly affected by cruelty to animals. This not only made him sick but put him also into an anti-clerical mood, for he held that the clergy were neglecting their duty in not preaching against such obvious evil. In literature, as might be expected, his taste was for the metaphysical; Dante's *Paradiso* was his favourite poetry. At one period of his life he himself composed verses of a reflective kind, and it may be of interest to reprint a piece of his which has survived. I do not pretend that it is great poetry, but poetry of any kind by great mathematicians is not common, and this has at least the merit of showing something of the author. It is a *Sonnet to the Number Three*:

When the great Maker, on creation bent,
Thee from thy brethren chose, and framed by thee
The world to sense revealed, yet left it free,
To those whose intellectual gaze intent
Beyond the veil phenomenal is sent,
Space diverse, systems manifold to see
Revealed by thought alone; was it that we,
In whose mysterious spirits thus are blent
Finite of sense and infinite of thought,
Should feel how vast, how little is our store—
As yon excelling arch with orbs deep fraught
To the light wave that dies along the shore—
Till from our weakness and our strength may rise
One worship unto Him, the only wise?

Boole's behaviour during his last illness was characteristic of the man. Having caught a cold which turned to pneumonia,

he insisted on calling in a doctor who had recently been dismissed from a professorship of medicine at Queen's College for some irregularity of conduct. This he did in order to show friendship and to give pleasure to a man who was in distress. When his mind had been wandering in fever, he told his wife that the whole universe seemed to be spread before him like a great black ocean, where there was nothing to see and nothing to hear, except that at intervals a silver trumpet seemed to sound across the waters "For ever, O Lord, Thy word is settled in heaven." And as he lay in bed on the borders of delirium, all the little sounds of the house, such as the creaking of doors, resolved themselves into a chant of these words, which expressed for him the excellence of mathematical truth. One of his last wishes was that his children should not be allowed to fall into the hands of those who were commonly thought religious. He died on the 8th of December, 1864.

(2)

The mathematical logic of our day has been produced by the junction of two streams of thought. The first, which can be traced back to Leibniz, is an attempt to improve the presentation of logic by mathematical technique and in particular by the use of algebraic symbolism. The second, which arose in the nineteenth century, is an attempt to free mathematics itself from paradoxes by precise formulation of its postulates and procedures. It would be a mistake, however, to suppose that the contributions of the two streams can be clearly distinguished. The expression of truths of logic in symbolism derived from mathematics would be a trivial game if it served no ulterior purpose.¹ But those who tried it were always moved by a desire to bring out certain affinities which they thought they could detect between propositions commonly called logical and others called algebraic. In short, they were already interested in the kind of formal study which is required for the solution of questions about the foundations of mathematics.

It is unlikely that Boole knew anything of the work of his predecessors in this field when he first conceived the idea of an algebra of logic. But I think Venn went too far when he said that Boole never had any suspicion that others had tried his plan before him.² He was well read in the history of logic and

¹ I confess that I find the phrase "symbolic logic" rather irritating. We might as well talk of symbolic chemistry.

² *Symbolic Logic*, p. xxx.

philosophy in general. A passage in the *Laws of Thought* implies, for example, that he was acquainted with Wallis's *Institutio Logicae* of 1687 when he wrote the *Mathematical Analysis of Logic*;¹ and we know that in the interval between the two works he searched systematically for anything which would help in the elaboration of his own ideas. He may very well have missed Lambert, for that logician's writings were not easily accessible; but he certainly read some of Leibniz's works,² and if he used the Erdmann edition of 1840, he may have seen there the *Non Inelegans Specimen Demonstrandi in Abstractis*, which was the only important piece of Leibniz on mathematical logic then generally available. But it is useless to speculate on the matter. If he read any of the published works of his predecessors, he cannot have got much from them, because there was not much to be learnt there beyond the general suggestion that algebraic technique might be applied to logic—a suggestion which had already occurred to him while he was still a youth. Leibniz had, indeed, done more than this towards the construction of a *calculus ratiocinator*, but his most interesting papers lay still unread in the library at Hanover.³

It is sometimes thought strange that mathematical logic made comparatively little progress in the century and a half after Leibniz had announced the programme. Lambert did some good work in the middle of the eighteenth century, but after that there was nothing of note until the middle of the nineteenth century. Venn has suggested that Kant was responsible for the neglect into which the study fell.⁴ Although I hold no brief for the defence of Kant, I think this is a little unfair. No doubt Kant's influence was hostile to the development of mathematical logic, but other conditions also were unfavourable; in particular algebra was not yet sufficiently developed. It is true that Lambert anticipated some of the usages adopted by Boole, but he did not succeed in working out a calculus of logic, and the analogies introduced by his symbols were in part misleading. It is fairly easy to see that there is some resemblance between the disjunction and conjunction of concepts on the one hand and the addition and multiplication of numbers on the other, but it is not so easy to formulate the resemblance precisely and use

¹ *L. of T.*, p. 176. Wallis's work is not mathematical, although he was a distinguished mathematician.

² *L. of T.*, p. 240.

³ For the nearness of Leibniz's ideas to those of Boole see L. Couturat, *La Logique de Leibniz* (1901), ch. viii.

⁴ *Symbolic Logic*, p. xxxvii.

it as the basis of an algebra of logic. Now it was just this that Boole achieved, and it was no accident that there had been considerable development of algebra shortly before he wrote.

We have seen already that he was interested in the calculus of operators. It is probable that he was also well acquainted with a paper of Gregory "On the Real Nature of Symbolical Algebra" which appeared in the *Edinburgh Philosophical Transactions* of 1838, and with four papers of De Morgan on "The Foundation of Algebra" which appeared in the *Transactions of the Cambridge Philosophical Society* between 1839 and 1844, and that he had seen Hamilton's first paper on quaternions which appeared in the *Philosophical Magazine* of 1844.¹ From these sources it was possible to collect two important discoveries: (i) that there could be an algebra of entities which were not numbers in any ordinary sense, and (ii) that the laws which hold for types of numbers up to and including complex numbers need not all be retained together in an algebraic system not applicable to such numbers. With his genius for generalisation, Boole saw that an algebra could be developed as an abstract calculus capable of various interpretations. The view of logic which he derived from this insight is clearly stated in the opening section of his *Mathematical Analysis of Logic*: "They who are acquainted with the present state of the theory of Symbolical Algebra, are aware that the validity of the processes of analysis does not depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination. Every system of interpretation which does not affect the truth of the relations supposed, is equally admissible, and it is thus that the same processes may, under one scheme of interpretation, represent the solution of a question on the properties of numbers, under another, that of a geometrical problem, and under a third, that of a problem of dynamics or optics. . . . We might justly assign it as the definitive character of a true Calculus, that it is a method resting upon the employment of Symbols, whose laws of combination are known and general, and whose results admit of a consistent interpretation. That to the existing forms of Analysis a quantitative interpretation is assigned, is the result of the circumstances by which those forms were determined, and is not to be construed into a universal condition of Analysis. It is upon the foundation of this general principle, that I purpose to

¹ On the other hand, it is almost certain that he did not know Grassmann's *Ausdehnungslehre*, which was also published in 1844.

establish the Calculus of Logic, and that I claim for it a place among the acknowledged forms of Mathematical Analysis, regardless that in its objects and in its instruments it must at present stand alone".¹

In the exposition of Boole's system it will be convenient to begin with that interpretation from which, in all probability, he obtained his conception of a calculus of logic. Let us suppose that symbols such as " x ", " y " and " z " stand for classes, and that the use of the symbol " $=$ " between two class symbols indicates that the two classes concerned have the same members. Then the *intersection* of two classes, that is to say, the class consisting of all the things which belong to both those classes, may be represented by the complex symbol " xy ". This convention is suggested by the way in which we string adjectives together when we are trying to specify some narrowly defined class. We may speak, for example, of the class of large, red, square things. Boole, indeed, does not distinguish very sharply between adjectives and class symbols; sometimes he calls the letters " x ", " y ", " z ", etc., *elective symbols*, thinking of them as symbols which elect (*i.e.* select) certain things for attention. Among all possible classes there are, however, two limiting cases for which it is convenient to have special symbols. These are the *universe* class, or the class of things in general, and the *null* class, or the class of which nothing is a member. For these Boole employs respectively the signs " 1 " and " 0 ". These are not to be taken for the moment as numerals, but it will be noticed that according to the explanations just given

$$x1 = x$$

and

$$0x = 0,$$

whatever x may be.

There are obvious resemblances between this symbolism for the intersection of classes and the ordinary symbolism for the multiplication of numbers, and for this reason it is quite usual to call the intersection of two classes their logical product. There is, however, a very important peculiarity of the symbolism for intersection, which is not to be found in the general rules for multiplication. If x is a class, its intersection with itself is the same class, *i.e.*

$$xx = x.$$

¹ *M. A. of L.*, pp. 3-4. The relation of Boole's logic to the mathematics of his day is discussed by E. Nagel in an essay on "Impossible Numbers" contributed to *Studies in the History of Ideas* III (Columbia U. P., 1935).

In his *Mathematical Analysis of Logic* Boole often expresses this truth by the formula :

$$x^n = x,$$

saying that it is the distinguishing feature of his calculus. We shall meet it again presently in another shape.

Division appears in numerical algebra as the inverse of multiplication, but Boole does not allow an operation analogous to division when he is talking of classes. He points out that if x , y and z are classes we cannot argue from the equation :

$$xz = yz$$

to the conclusion :

$$x = y.$$

The class of bachelor archdeacons may perhaps be co-extensive with the class of red-haired archdeacons, but it does not follow from this that all bachelors are red-haired. It should be remembered, however, that even in numerical algebra there are limits to the proper use of the operation of division. We cannot argue from the equation :

$$2 \times 0 = 3 \times 0$$

to the conclusion :

$$2 = 3.$$

So far, then, it may be possible after all to find some analogue of division in the theory of classes, and Boole himself has suggested that the analogue may be abstraction. Let us suppose that x , y and z are classes related by the equation :

$$x = yz.$$

The suggestion is that we should write :

$$\frac{x}{y} = z$$

as an expression for the fact that z is a class we reach by abstracting from the membership of x the restriction of being included in y . But it is important to notice two limits to this convention.

First, the expression " $\frac{x}{y}$ " can have no meaning at all in the calculus of classes if the class x is not in fact a part of the class y . This is like the observation that " $\frac{x}{y}$ " can have no meaning in an algebra of integers, if x is not a multiple of y . Secondly, the expression " $\frac{x}{y}$ " is generally indeterminate in the calculus of classes : for there may be many different classes whose intersections with y are all co-extensive with x . This is a much more

serious limitation of the use of the suggested notation, and accounts, I think, for Boole's statement that " $\frac{x}{y}$ " cannot be interpreted in logic.

In the algebra of classes the expression " $x + y$ " may be used to signify the class of things which belong to the class x or to the class y . But the word "or" has two distinguishable meanings in ordinary English; it is used sometimes in an inclusive and sometimes in an exclusive sense. Most of Boole's successors in mathematical logic have taken " $x + y$ " for the *union* (or logical sum) of the classes x and y in an inclusive sense, *i.e.* so that $x + y$ may contain xy . This convention has considerable advantages from the formal point of view. It allows us to assert :

$$x + x = x$$

as a parallel to the equation :

$$xx = x,$$

and so to work out the whole calculus in accordance with a principle of duality for union and intersection. In particular, we can adopt De Morgan's two rules and write :

$$\begin{array}{c} \overline{xy} = \bar{x} + \bar{y} \\ \overline{x + y} = \bar{x}\bar{y}, \end{array}$$

with " \bar{x} " standing for the class complementary to x , *i.e.* the class of all things which do *not* belong to x . But Boole chooses the other alternative, and decides that, if " x " and " y " stand for classes, we are not to write " $x + y$ " unless x and y are mutually exclusive. In order to make it clear that this condition is satisfied, he sometimes expresses a logical sum in the form " $x + \bar{x}y$ ", where " \bar{x} " has the sense explained above.

His reason for adopting the exclusive sense for "+" was probably a wish to be able to use "—" as its inverse. We have here a situation something like that explained above in connexion with the sign " $\frac{x}{y}$ ". When x , y and z are classes, it is tempting to suppose that from the equation :

$$x = y + z$$

we can derive another equation :

$$x - y = z.$$

But there are two limitations to this usage in the algebra of classes. In the first place we can attach no meaning to the expression " $x - y$ " if the class y is not contained in the class x . This is like the observation that " $x - y$ " is meaningless in an algebra of positive numbers if y is greater than x . Secondly

“ $x - y$ ” will be indeterminate if “+” is taken in an inclusive sense. For with the inclusive sense a statement of the form :

$$x = y + z$$

may be quite compatible with a statement of the form :

$$x = y + w,$$

although “ z ” and “ w ” do not stand for co-extensive classes. Whatever his motive, Boole does in fact adopt the exclusive use for “+” and make free use of “-” as its inverse. In particular, he writes “ $1 - x$ ” for the class of things which do not belong to x and introduces “ \bar{x} ” merely as an abbreviation. This usage enables him to express the special principle of his system in the form :

$$x(1 - x) = 0.$$

Sometimes he derives this from the equation :

$$x = x^2,$$

which is a special case of his index law ; but sometimes he prefers to regard it as basic, remarking that it is a formulation of the principle of non-contradiction, which both Aristotle and Leibniz described as the most fundamental of all principles.

Boole's notation contains no provision for individual variables or quantifiers to bind them (*i.e.* universal and existential operators), but it is nevertheless sufficient for the expression of the A, E, I and O propositions of traditional logic. Supposing “ x ” to stand for the class of X things, we have the following scheme :

<i>Every X is Y</i>	...	$x(1 - y) = 0$
<i>No X is Y</i>	...	$xy = 0$
<i>Some X is Y</i>	...	$xy \neq 0$
<i>Some X is not Y</i>	...	$x(1 - y) \neq 0$

Each of the four propositions can be expressed in various other ways which are algebraically equivalent ; I have chosen the four expressions listed above because they seem to me the simplest. It will be noticed, however, that in this scheme particular propositions are expressed by inequalities, whereas universal propositions are expressed by equations. I have introduced here a distinction which is not recognised by Boole. He assumes that all the traditional types of categorical propositions must be expressible by equations, and therefore writes instead :

<i>Some X is Y</i>	...	$xy = v$
<i>Some X is not Y</i>	...	$x(1 - y) = v$

The letter “ v ”, which he introduces here for the special purpose of expressing particular propositions, seems to correspond to the

English word "some"; but it is said to stand for a class "indefinite in all respects but one", namely, that it contains a member or members (*i.e.* is not co-extensive with the null class). This explanation is very unsatisfactory. If some X thing is Y , the class xy does indeed contain at least one member; but we cannot assert this by equating the class xy with a class whose sole defining characteristic is that of containing a member, for there is no such class. Within Boole's system the letter " v " can be manipulated in some respects as though it were a class symbol; but this is a defect rather than a merit of the notation, for it suggests mistaken inferences. We may be tempted, for example, to suppose that " $x = v$ " and " $y = v$ " together entail " $x = y$ ". Boole himself does not fall into such fallacies, but that is because he observes restrictions on the use of " v " which are inconsistent with his own description of it as a class symbol.¹

For the interpretation of the system in terms of classes and operations on classes it is clear that the following principles, which are explicitly or implicitly assumed by Boole, all hold good :

- (1) $xy = yx$
- (2) $x + y = y + x$
- (3) $x(y + z) = xy + xz$
- (4) $x(y - z) = xy - xz$
- (5) If $x = y$, then $xz = yz$
- (6) If $x = y$, then $x + z = y + z$
- (7) If $x = y$, then $x - z = y - z$
- (8) $x(1 - x) = 0$.

Of these the first seven are similar in form to rules of ordinary numerical algebra. What distinguishes the system is the eighth principle. But Boole has pointed out that even this can be interpreted numerically with a suitable restriction of the values for the symbol " x ". He states his discovery in the following words : "Let us conceive, then, of an Algebra in which the symbols x , y , z , etc., admit indifferently of the values 0 and 1,

¹ In the *Laws of Thought* (p. 61) the symbol " v " is used even in the formulation of universal propositions. For "Every X thing is Y " Boole writes there " $x = vy$ ". This is presumably an attempt to allow for Hamilton's doctrine of the quantification of the predicate. But he himself shows later (p. 104) that " v " can be eliminated from the expression of universal propositions, and I have therefore followed the simpler account given in the *Mathematical Analysis of Logic*. It is curious that Leibniz had thought of the same device : Conturat, *La Logique de Leibniz*, pp. 345-350.

and of these values alone. The laws, the axioms, and the processes, of such an algebra will be identical in their whole extent with the laws, the axioms, and the processes of an Algebra of Logic. Differences of interpretation will alone divide them. Upon this principle the method of the following work is established".¹ This passage, and others like it, have led some commentators to describe Boole's system as a two-valued algebra. But this is a mistake ; for when we treat the system as a calculus of classes, we do *not* assume that every class is co-extensive with the universe class or the null class. It is possible, indeed, to turn Boole's system into a two-valued algebra by adding a new principle :

(9) Either $x = 1$ or $x = 0$.

The resulting calculus can still be interpreted numerically (*i.e.* by taking "1" and "0" in their ordinary numerical senses) ; but it no longer admits an interpretation in terms of classes. Boole does not distinguish sharply between his original system and the narrower system which involves (9). This may be seen from the fact that he gives two other logical interpretations of his formulæ without remarking that one satisfies (9) whereas the other does not. These other interpretations have great interest for their own sake and deserve to be mentioned here.

In the *Mathematical Analysis of Logic* and again in the *Laws of Thought* Boole suggests a convention whereby " $x = 1$ " may be taken to mean that the proposition X is true and " $x = 0$ " that X is false. In accordance with this usage the truth values of more complicated propositions can be represented by functions of small letters, *e.g.* the truth value of the conjunction of X and Y by " xy " and the truth value of the exclusive disjunction of X and Y by " $x + y$ ". It will be seen that we have here all that is needed for an interpretation of Boole's system in terms of the truth values of propositions with "1" for truth and "0" for falsity. But this interpretation, like the numerical one, satisfies also the stricter system which involves principle (9).

Although the interpretation by reference to propositions is worked out by Boole in the way just noticed, it is introduced on each occasion by remarks which might lead us to expect something more interesting. In the *Mathematical Analysis of Logic* Boole writes : "To the symbols X , Y , Z , representative of propositions, we may appropriate the elective symbols x , y , z , in the following sense. The hypothetical Universe, 1, shall comprehend all conceivable cases and conjunctures of circumstances. The

¹ *L. of T.*, pp. 37-38.

elective symbol x attached to any subject expressive of such cases shall select those cases in which the Proposition X is true, and similarly for Y and Z . If we confine ourselves to the contemplation of a given proposition X , and hold in abeyance any other consideration, then two cases only are conceivable, *viz.*, first that the given proposition is true, and secondly that it is false. As these two cases together make up the Universe of the Proposition, and as the former is determined by the elective symbol x , the latter is determined by the elective symbol $1 - x$. But if other considerations are admitted, each of these cases will be resolvable into others, individually less extensive, the number of which will depend on the number of foreign considerations admitted. Thus if we associate the Propositions X and Y , the total number of conceivable cases will be found as exhibited in the following scheme.

Cases.	Elective Expressions.
1st X true, Y true	xy
2nd X true, Y false	$x(1 - y)$
3rd X false, Y true	$(1 - x)y$
4th X false, Y false	$(1 - x)(1 - y)$

... And it is to be noted that however few or many those circumstances may be, the sum of the elective expressions representing every conceivable case will be unity".¹ In the *Laws of Thought* Boole abandons this very promising suggestion and proposes instead that " x " should be taken to stand for the *time* during which the proposition X is true, with " 1 " for eternity. Apparently he wishes to take literally the distinction of philosophers between eternal and contingent verities. In both works, however, he seems to be leading up to a scheme in which " 1 " would be taken to stand for truism and " 0 " for absurdity, with elective expressions of other propositions in between. Like the class interpretation, to which it corresponds closely, this scheme would not satisfy principle (9).

Perhaps Boole had some inkling of the possibility I have just mentioned, for at the end of his *Mathematical Analysis of Logic* he said rather obscurely that the theory of hypothetical propositions could be treated as part of the theory of probabilities, and in the *Laws of Thought* several chapters are devoted to the application of the new algebra to probabilities. I shall not attempt to reproduce Boole's discussion of this topic in his own phraseology, but content myself with saying that it amounts to

¹ *M. A. of L.*, p. 49.

an interpretation of the calculus according to which " x " may be taken as standing for the probability of the proposition X in relation to all the available information, say K . Adapting and simplifying Boole's symbolism, we have

$$\text{Prob}_K(X \text{ and } Y) = xy,$$

if X and Y are independent, given K , and

$$\text{Prob}_K(X \text{ or } Y) = x + y,$$

if X and Y are mutually exclusive. Clearly this scheme does not satisfy principle (9), for we cannot allow that every probability is equal to 1 or 0.

In an earlier paragraph I have quoted a statement by Boole that his method is founded on the identity of the calculus of logic with a numerical algebra of 0 and 1. Later in the same book he says emphatically: "We may in fact lay aside the logical interpretation of the symbols in the given equation; convert them into quantitative symbols, susceptible only of the values 0 and 1; perform upon them as such all the requisite processes of solution; and finally restore to them their logical interpretation".¹ We have seen that the numerical algebra of 0 and 1 is a narrower system than the algebra of classes, since it involves principle (9), which does not hold for classes. But for anything which can be proved in the algebra of 0 and 1 *without the use of principle (9)* there will, of course, be an analogue in the algebra of classes, and Boole does not in fact use principle (9), or anything which implies it, when he manipulates the symbols of his calculus.² If he sometimes allows himself to think of his symbols as numerical, that is merely a concession to custom, something which makes him feel at home with them. The essence of his method is really the derivation of consequences in abstract fashion, *i.e.* without regard to interpretation. He made this clear in the opening section of his *Mathematical Analysis of Logic*, although he sometimes forgot it in later utterances.

The fundamental process in the formal elaboration of Boole's system is one which he calls *development*. Let us suppose that " $f(x)$ " is an abbreviation for an expression involving " x " and possibly other elective symbols but apart from them only the usual algebraic signs we have already discussed. Then for suitable coefficients A and B we have

$$f(x) = Ax + B(1 - x).$$

And in order to determine the values of A and B we need only

¹ *L. of T.*, p. 70. The whole sentence is italicised in the original.

² In this respect his calculus has some resemblance to Brouwer's logic.

suppose x to take the values 1 and 0. For then we obtain by substitution the equations :

$$f(1) = A$$

and

$$f(0) = B,$$

from which we can infer that

$$f(x) = f(1)x + f(0)(1 - x).$$

This is said to be the development of " $f(x)$ " with respect to x .¹ Let us now suppose that " $\phi(x, y)$ " is an abbreviation for an expression which apart from the elective symbols " x " and " y " involves *only* the usual algebraic signs. Regarding this first as a function of " x ", we can say :

$$\phi(x, y) = \phi(1, y)x + \phi(0, y)(1 - x).$$

Considering the result as a function of " y " we can then go on to write :

$$\phi(x, y) = \phi(1, 1)xy + \phi(1, 0)x(1 - y) + \phi(0, 1)(1 - x)y + \phi(0, 0)(1 - x)(1 - y),$$

which is the complete development of " $\phi(x, y)$ ". It is easy to see how functions of a larger number of elective symbols may be developed in similar fashion. If, for example, we have to do with an elective function of three arguments, the development will contain $2^3 = 8$ terms, each consisting of a coefficient equal to 0 or 1 and a product from the following table :

$$\begin{array}{l}xyz \\ (1 - x)yz \\ x(1 - y)z \\ xy(1 - z) \\ (1 - x)(1 - y)z \\ (1 - x)y(1 - z) \\ x(1 - y)(1 - z) \\ (1 - x)(1 - y)(1 - z).\end{array}$$

Those terms whose coefficients are equal to 0 can naturally be omitted without alteration to the sense of the whole expression.

Since any elective equation can be brought into the form :
 $\phi(x, y \dots) = 0,$

it is clear that it can also be expressed by the equation of a sum of terms to 0. From such an equation it can then be inferred that each of the terms included in the sum is separately equal

¹ In the *Mathematical Analysis of Logic* (p. 60) Boole treats his development formula as a degenerate case under Maclaurin's theorem about the expansion of functions in ascending powers, but he relegates this observation to a footnote in the *Laws of Thought*.

to 0 (for negative terms are not admissible). On the other hand, if we have two elective equations :

$$\phi(x, y \dots) = 0$$

and :

$$\psi(x, y \dots) = 0,$$

their full force can always be rendered in one equation :

$$\chi(x, y \dots) = 0$$

which includes among the terms of its developed form every term included in the developed form of either of the other two. It is then said that the two given equations have been *reduced to one*.

On this basis it is possible to define another operation of importance, namely, *solution*. Let us suppose that the given information is all contained in one equation :

$$f(x) = 0$$

where " $f(x)$ " is an abbreviation for a function involving elective symbols other than " x ", and that it is desired to express x in terms of those other symbols, *i.e.* by an equation :

$$x = \phi(y, z \dots).$$

Developing our original equation with respect to x we have :

$$f(1)x + f(0)(1 - x) = 0.$$

This can be rewritten in the form :

$$[f(1) - f(0)]x + f(0) = 0,$$

and it is then easy to infer that

$$x = \frac{f(0)}{f(0) - f(1)}.$$

It will be noticed that Boole introduces here the operation of division to which he has so far assigned no interpretation in his calculus of logic. In order to make general provision for the solution of elective equations he now lays down some special rules of interpretation, or decoding, as we may call it. The task is not so difficult as it might appear at first sight. When an expression of the form " $\frac{f(0)}{f(0) - f(1)}$ " is developed with respect

to all the elective symbols it involves, the result is a sum of products, and the sign of division appears only in the various coefficients. Each of these must have one of the four forms :

$$\begin{array}{cccc} 1 & 0 & 1 & 0 \\ \overline{1} & \overline{1} & \overline{0} & \overline{0} \end{array}$$

The first two raise no difficulty, because they can be reduced to

"1" and "0" respectively, and it can be shown that the third will never occur except as coefficient to a product which is separately equal to 0. The last is taken by Boole to be a perfectly indeterminate symbol of quantity, corresponding to "all, some or none". Sometimes he equates it with the "v" which he uses in the expression of particular propositions, but this is unsatisfactory, since "v" is supposed to stand for a class which is not null. We might perhaps use "w" instead as an abbreviation for $\frac{0}{0}$. Thus if the application of the rules of solution has produced a result of the form :

$$x = \frac{1}{1} P + \frac{0}{1} Q + \frac{1}{0} R + \frac{0}{0} S,$$

where P, Q, R and S are the various products to be considered for inclusion in the expression of x , we can write :

$$x = P + w S.$$

According to the class interpretation this means that the class x consists of the class P together with an indeterminate part (all, some or none) of the class S .

Finally, it is possible to lay down rules for *elimination*. Let us suppose that $f(x) = 0$ and that it is desired to find what relations, if any, hold between the other classes symbolised, independently of x . By the process of solution we get :

$$x = \frac{f(0)}{f(0) - f(1)}$$

and from this we can obtain :

$$1 - x = -\frac{f(1)}{f(0) - f(1)}.$$

But according to the distinctive principle of the calculus

$$x(1 - x) = 0.$$

$$\text{Therefore } -\frac{f(0)f(1)}{[f(0) - f(1)]^2} = 0$$

$$\text{i.e. } f(0)f(1) = 0.$$

If the working out of the left side of this equation yields only :

$$0 = 0,$$

the premiss covers no relations independent of x . If, however, it yields something of the form :

$$AB = 0$$

where neither A nor B is null, that is as much as to say that the

equation " $f(x) = 0$ " covers some relation into which x enters only vacuously.

The verification of all these assertions for the various possible interpretations of the symbolism would take too long, but for the benefit of any reader who has the same attitude towards mathematical logic as the Dauphin had towards geometry I give my word that they are all in order. In particular it is possible to carry out most of the reasoning which is required in the calculus of classes by reduction, elimination and solution or some combination of these. Syllogism, for example, can be presented as the reduction of two equations to one, followed by elimination of the middle term and solution for the subject term of the conclusion.

(3)

Boole's reputation is higher now than ever before, but it would be wrong to suggest that he was neglected in his own lifetime or during the next generation. De Morgan, Jevons, Venn, Schröder and Peirce all recognised the importance of his work, and there were evidently many others who were impressed although they did not understand it. As early as 1859 there appeared a book in which a Dublin clergyman called Tresham Dames Gregg tried to apply Boolean methods (or what he took for such) to theology.¹ And during the next half century many of the new books on logic contained a chapter or appendix against "the equational logic". It is correct, however, to say that there has been a change of emphasis in the thought of those who praise Boole, and I wish, therefore, to consider in conclusion why Boole's work is important enough to deserve commemoration after a century.

To his contemporaries and immediate successors it seemed that Boole had supplied a complete or nearly complete system of symbolic logic. He had shown that his calculus could be interpreted by reference to classes, to the truth values of propositions or to probabilities, and these topics exhausted the field of traditional logic. Presently, indeed, logicians became interested in the theory of relations; but it was found that there, too, the calculus could be applied. What more could be desired? According to our ideas there was one serious shortcoming in Boole's calculus, considered as a system of logic; it contained no quantifiers, and therefore could not deal with some of the most interesting

¹ There is an amusing notice of this in De Morgan's *Budget of Paradoxes*, p. 297.

and important notions of science. We have seen that the “*v*” symbol is unsatisfactory for the expression of existential propositions, but the situation is even more serious than it appears at first sight. In Boole’s system there is no way at all by which we can exhibit clearly what is involved in a multiply general proposition such as the assertion that an infinite sequence $T_1, T_2, T_3 \dots$ is convergent. In modern symbolism this can be expressed by the formula :

$$(\exists l)(\epsilon)(\exists n)(r) \cdot r > n \supset |T_r - l| < \epsilon,$$

where the small letters after the stop are all variables bound by the operators before the stop ; but these devices are due to Frege and his successors. We must therefore allow that Boole’s calculus was by no means a complete system of logic.

In saying this I do not mean to imply that Boole’s contributions to logical analysis were trivial. On the contrary we can see now that he introduced two notions which are of the greatest importance, namely, that of a *truth-function* and that of a *normal form*. When Boole wrote of an elective function, he meant an expression which involved various elective symbols and apart from them only the symbols of conjunction, disjunction and negation, or symbols definable by reference to these. And he saw clearly that any such function could be represented as the disjunction of a certain number of conjunctions selected from among the various possibilities “ $xy \dots$ ”, “ $\bar{x}y \dots$ ”, “ $x\bar{y} \dots$ ”, etc. I have already quoted a passage from which it is a short step to the construction of a truth value table.¹

Towards the end of his life Boole said that he was dissatisfied with the exposition and arrangement of his *Laws of Thought*, and that he wished he had spent twice as long in working out the ideas first presented in his *Mathematical Analysis of Logic*. It is interesting to speculate on the changes he might have made by further revision. We have seen that without departing far from his general plan he could have made a more elegant system by adopting the principle :

$$x + x = x,$$

which produces a parallelism between the rules for disjunction and those for conjunction. His immediate followers introduced this change, and at the same time got rid of his rather awkward provision for numerical coefficients other than 0 or 1. I suspect, however, that what he had in mind may have been no alteration

¹ The tabular method of presentation was introduced by Jevons. Venn preferred diagrams of overlapping regions (*i.e.* topological models). Each thought of that which he was trying to elucidate as the principal novelty in Boole’s logic. The expression “normal form” is taken from Hilbert.

of the formal side of his work, but an elaboration of the metaphysical passages to which he attached considerable importance. If this was his wish, I think it unlikely that he would have produced a better book by working longer. For it seems to me that he was on a wrong track when he began to look for the basis of logic in the constitution of the human intellect. I wish to suggest, indeed, that the chief merit of his book is that it led to the liberation of logic from the dominance of psychology and epistemology, and so brought about its revival as an independent science.

The sixteenth and seventeenth centuries, which brought a renaissance of most studies, were on the whole a time of decadence for formal logic. In spite of the great claims which had been made for it in the Middle Ages, it seemed obviously inadequate as an instrument for the advancement of science, whether *a priori* or empirical ; and so it fell into contempt, together with mediæval metaphysics, although it continued to have a place in the educational scheme. From the time of Descartes onwards philosophers occupied themselves principally with problems about the status of minds in the universe and their ability to gain knowledge. By the end of the eighteenth century this shift of interest had gone so far that all philosophical disciplines were supposed to be concerned with the mind, and the word "metaphysics" was often used as a synonym for "mental science". It is therefore not surprising that logic came to be regarded as the study of thinking. The first break with this confused tradition was made by Bolzano (himself a philosopher-mathematician), but it was Boole's work which showed clearly by example that logic could be studied profitably without any reference to the processes of our minds. He believed, no doubt, that he was dealing with laws of thought in some psychological sense of that confused expression ;¹ but he was in fact dealing with some of the most general laws of thinkables. His work was by no means the final system which his first admirers thought it to be, but it was a solid beginning.

Mr. Russell once said "Pure mathematics was discovered by Boole, in a work which he called *The Laws of Thought*".² Like

¹ Hence his wife's notion that his "message" had to do with "psychic science".

² *Mysticism and Logic*, p. 74; quoted by E. T. Bell in *Men of Mathematics*, p. 483. For a more sober appreciation of Boole by Mr. Russell see *Our Knowledge of the External World*, pp. 49-50. It so happens that Boolean algebra has a special interest to mathematicians in our day; for the theory of lattices which has grown out of his work is said to offer hope of large-scale unification.

many other good epigrams, this is a gross exaggeration ; but anyone who remembers Mr. Russell's account of mathematics can understand what prompted him to make the remark. He maintains that pure mathematics is identical with logic, and when he says this, the logic to which he refers is, of course, *mathematical logic*, *i.e.* logic of the kind Boole discovered. But even those who reject Mr. Russell's account of mathematics may still allow that there is some truth in the epigram. I have quoted in the earlier sections of this paper some passages where Boole suggested the notion of a mathematical calculus as a deductive system in which the theorems are not necessarily concerned with number or quantity, but follow from the postulates in virtue of their form and hold for any entities that satisfy the postulates (*i.e.* that provide a consistent interpretation). This conception has come to be generally accepted, but it was Boole who produced the widening of view by his construction of a calculus of classes ; and he therefore deserves the credit of having discovered, if not pure mathematics, then a new view of the science.

V.—CAN GOD'S EXISTENCE BE DISPROVED?

BY J. N. FINDLAY.

THE course of philosophical development has been full of attempted proofs of the existence of God. Some of these have sought a basis in the bare necessities of thought, while others have tried to found themselves on the facts of experience. And, of these latter, some have founded themselves on *very general facts*, as that something exists or that something is in motion, while others have tried to build on *highly special facts*, as that living beings are put together in a purposive manner, or that human beings are subject to certain improbable urges and passions, such as the zeal for righteousness, the love for useless truths and unprofitable beauties, as well as the many specifically religious needs and feelings. The general philosophical verdict is that none of these "proofs" is truly compelling. The proofs based on the necessities of thought are universally regarded as fallacious: it is not thought possible to build bridges between mere abstractions and concrete existence. The proofs based on the general facts of existence and motion are only felt to be valid by a minority of thinkers, who seem quite powerless to communicate this sense of validity to others. And while most thinkers would accord weight to arguments resting on the special facts we have mentioned, they wouldn't think such arguments successful in ruling out a vast range of counter-possibilities. Religious people have, in fact, come to acquiesce in the total absence of any cogent proofs of the Being they believe in: they even find it positively satisfying that something so far surpassing clear conception should also surpass the possibility of demonstration. And non-religious people willingly mitigate their rejection with a tinge of agnosticism: they don't so much deny the existence of a God, as the existence of good reasons for believing in Him. We shall, however, maintain in this essay that there isn't room, in the case we are examining, for all these attitudes of tentative surmise and doubt. For we shall try to show that the Divine Existence can only be conceived, in a religiously satisfactory manner, if we also conceive it as something inescapable and necessary, whether for thought or reality. From which it follows that our modern denial of necessity or rational evidence for such an existence amounts to a demonstration that there cannot be a God.

Before we develop this argument, we must, however, give

greater precision to our use of the term "God". For it is possible to say that there are nearly as many "Gods" as there are speakers and worshippers, and while existence may be confidently asserted or denied of *some* of them, we should feel more hesitant in the case of others. It is one thing, plainly, to pronounce on God's existence, if He be taken to be some ancient, shapeless stone, or if we identify Him with the bearded Father of the Sistine ceiling, and quite another matter, if we make of him an "all-pervasive, immaterial intelligence", or characterise him in some yet more negative and analogical manner. We shall, however, choose an indirect approach, and pin God down for our purposes as the "adequate object of religious attitudes". Plainly we find it possible to gather together, under the blanket term "religious", a large range of cases of possible action, linked together by so many overlapping¹ affinities that we are ready to treat them as the varying "expressions" of a single "attitude" or "policy". And plainly we find it possible to indicate the character of that attitude by a number of descriptive phrases which, though they may err individually by savouring too strongly of particular cases, nevertheless permit us, in their totality, to draw a rough boundary round the attitude in question. Thus we might say, for instance, that a religious attitude was one in which we tended to abase ourselves before some object, to defer to it wholly, to devote ourselves to it with unquestioning enthusiasm, to bend the knee before it, whether literally or metaphorically. These phrases, and a large number of similar ones, would make perfectly plain the sort of attitude we were speaking of, and would suffice to mark it off from cognate attitudes which are much less unconditional and extreme in their tone. And clearly similar phrases would suffice to fix the boundaries of religious *feeling*. We might describe religious frames of mind as ones in which we felt ready to abase ourselves before some object, to bend the knee before it, and so forth. Here, as elsewhere, we find ourselves indicating the *felt* character of our attitudes, by treating their inward character as, in some sense, a concentrated and condensed substitute for appropriate lines of action, a way of speaking that accords curiously with the functional significance of the inward.² But not only do we incorporate, in the meanings of our various names for attitudes, a

¹ This word is added to avoid the suggestion that there must be *one* pervasive affinity linking together all the actions commonly called "religious".

² Whatever the philosophical "ground" for it may be, this plainly is the way in which we *do* describe the "inner quality" of our felt attitudes.

reference to this readiness for appropriate lines of action : we also incorporate in these meanings a reference to *the sorts of things or situations to which these attitudes are the normal or appropriate responses*. For, as a matter of fact, our attitudes are not indifferently evoked in *any* setting : there is a range of situations in which they normally and most readily occur. And though they may at times arise in circumstances which are not in this range, they are also readily dissipated by the consciousness that such circumstances *are* unsuitable or unusual. Thus fear is an attitude very readily evoked in situations with a character of menace or potential injury, and it is also an attitude very readily allayed by the clear perception that a given situation isn't really dangerous. And anger, likewise, is an attitude provoked very readily by perverse resistance and obstructive difficulty in some object, and is also very readily dissipated, even in animals, by the consciousness that a given object is innocent of offence. All attitudes, we may say, *presume* characters in their objects, and are, in consequence, strengthened by the discovery that their objects *have* these characters, as they are weakened by the discovery that they really haven't got them. And not only do we find this out empirically : we also incorporate it in the *meanings* of our names for attitudes. Thus attitudes are said to be "normal", "fully justified" and so forth, if we find them altered in a certain manner (called "appropriate") by our knowledge of the actual state of things, whereas we speak of them as "queer" or "senseless" or "neurotic", if they aren't at all modified by this knowledge of reality. We call it abnormal, from this point of view, to feel a deep-set fear of mice, to rage maniacally at strangers, to greet disasters with a hebephrenic giggle, whereas we think it altogether normal to deplore deep losses deeply, or to fear grave dangers gravely. And so an implicit reference to some standard object—which makes an attitude either normal or abnormal—is part of what we ordinarily mean by all our names for attitudes, and can be rendered explicit by a simple study of usage. We can consider the circumstances in which ordinary speakers would call an attitude "appropriate" or "justified". And all that philosophy achieves in this regard is merely to push further, and develop into more considered and consistent forms, the implications of such ordinary ways of speaking. It can enquire whether an attitude would still seem justified, and its object appropriate, after we had reflected long and carefully on a certain matter, and looked at it from every wonted and unwonted angle. And such consideration may lead philosophers to a different and more reasoned notion of the

appropriate objects of a given attitude, than could be garnered from our unreflective ways of speaking. And these developments of ordinary usage will only seem unfeasible to victims of that strange modern confusion which thinks of attitudes exclusively as hidden processes "in our bosoms", with nothing but an adventitious relation to appropriate outward acts and objects.

How then may we apply these notions to the case of our religious attitudes? Plainly we shall be following the natural trends of unreflective speech if we say that religious attitudes presume *superiority* in their objects, and such superiority, moreover, as reduces us, who feel the attitudes, to comparative nothingness. For having described a worshipful attitude as one in which we feel disposed to bend the knee before some object, to defer to it wholly, and the like, we find it natural to say that such an attitude can only be fitting where the object revered *exceeds* us very vastly, whether in power or wisdom or in other valued qualities. And while it is certainly possible to worship stocks and stones and articles of common use, one does so usually on the assumption that they aren't merely stocks and stones and ordinary articles, but the temporary seats of "indwelling presences" or centres of extraordinary powers and virtues. And if one realises clearly that such things *are* merely stocks and stones or articles of common use, one can't help suffering a total vanishing or grave abatement of religious ardour. To feel religiously is therefore to presume surpassing greatness in some object: so much characterises the attitudes in which we bow and bend the knee, and enter into the ordinary meaning of the word "religious". But now we advance further—in company with a large number of theologians and philosophers, who have added new touches to the portrait of deity, pleading various theoretical necessities, but really concerned to make their object worthier of our worship—and ask whether it isn't wholly anomalous to worship anything *limited* in any thinkable manner. For all limited superiorities are tainted with an obvious relativity, and can be dwarfed in thought by still mightier superiorities, in which process of being dwarfed they lose their claim upon our worshipful attitudes. And hence we are led on irresistibly to demand that our religious object should have an *unsurpassable* supremacy along all avenues, that it should tower *infinitely* above all other objects. And not only are we led to demand for it such merely quantitative superiority: we also ask that it shouldn't stand surrounded by a world of *alien* objects, which owe it no allegiance, or set limits to its influence. The

proper object of religious reverence must in some manner be *all-comprehensive* : there mustn't be anything capable of existing, or of displaying any virtue, without owing all of these absolutely to this single source. All these, certainly, are difficult requirements, involving not only the obscurities and doubtful significance of the infinite, but also all the well-worn antagonisms of the immanent and transcendent, of finite sinfulness and divine perfection and preordination, which centuries of theological brooding have failed to dissipate. But we are also led on irresistibly to a yet more stringent demand, which raises difficulties which make the difficulties we have mentioned seem wholly inconsiderable : we can't help feeling that the worthy object of our worship can never be a thing that merely *happens* to exist, nor one on which all other objects merely *happen* to depend. The true object of religious reverence must not be one, merely, to which no *actual* independent realities stand opposed : it must be one to which such opposition is totally *inconceivable*. God mustn't merely cover the territory of the actual, but also, with equal comprehensiveness, the territory of the possible. And not only must the existence of *other* things be unthinkable without Him, but His own non-existence must be wholly unthinkable in any circumstances. There must, in short, be no conceivable alternative to an existence properly termed "divine" : God must be wholly inescapable, as we remarked previously, whether for thought or reality. And so we are led on insensibly to the barely intelligible notion of a Being in Whom Essence and Existence lose their separateness. And all that the great mediæval thinkers really did was to carry such a development to its logical limit.

We may, however, approach the matter from a slightly different angle. Not only is it contrary to the demands and claims inherent in religious attitudes that their object should *exist* "accidentally" : it is also contrary to those demands that it should *possess its various excellences* in some merely adventitious or contingent manner. It would be quite unsatisfactory from the religious standpoint, if an object merely *happened* to be wise, good, powerful and so forth, even to a superlative degree, and if other beings had, *as a mere matter of fact*, derived their excellences from this single source. An object of this sort would doubtless deserve respect and admiration, and other quasi-religious attitudes, but it would not deserve the utter self-abandonment peculiar to the religious frame of mind. It would deserve the *δούλεια* canonically accorded to the saints, but not the *λατρεία* that we properly owe to God. We might respect this object as

the crowning instance of most excellent qualities, but we should incline our head before the qualities and not before the person. And wherever such qualities were manifested, though perhaps less eminently, we should always be ready to perform an essentially similar obeisance. For though such qualities might be intimately characteristic of the Supreme Being, they still wouldn't be in any sense inalienably His own. And even if other beings had, in fact, derived such qualities from this sovereign source, they still would be *their own* qualities, possessed by them in their own right. And we should have no better reason to *adore* the author of such virtues, than sons have reason to adore superior parents, or pupils to adore superior teachers. For while these latter may deserve deep deference, the fact that we are coming to *participate* in their excellences renders them unworthy of our *worship*. Plainly a being that possesses and imparts desirable qualities—which other things might nevertheless have manifested though this source were totally absent—has all the utter inadequacy as a religious object which is expressed by saying that it would be *idolatrous* to worship it. Wisdom, kindness and other excellences deserve respect wherever they are manifested, but no being can appropriate them as its personal perquisites, even if it does possess them in a superlative degree. And so we are led on irresistibly, by the demands inherent in religious reverence, to hold that an adequate object of our worship must possess its various qualities in *some necessary manner*. These qualities must be intrinsically incapable of belonging to anything except in so far as they belong primarily to the object of our worship. Again we are led on to a queer and barely intelligible scholastic doctrine, that God isn't merely good, but is in some manner indistinguishable from His own (and anything else's) goodness.

What, however, are the consequences of these requirements upon the possibility of God's existence? Plainly they entail (for all who share a contemporary outlook) not only that there isn't a God, but that the Divine Existence is either senseless¹ or impossible. The modern mind feels not the faintest axiomatic force in principles which trace contingent things back to some necessarily existent source, nor does it find it hard to conceive that things should display various excellent qualities without deriving them from a source which manifests them supremely. Those who believe in necessary truths which aren't merely tautological, think that such truths merely connect the *possible*

¹ I have included this alternative, of which I am not fond, merely because so many modern thinkers make use of it in this sort of connexion.

instances of various characteristics with each other : they don't expect such truths to tell them whether there *will* be instances of any characteristics. This is the outcome of the whole mediæval and Kantian criticism of the Ontological Proof. And, on a yet more modern view of the matter, necessity in propositions merely reflects our use of words, the arbitrary conventions of our language. On such a view the Divine Existence could only be a necessary matter if we had made up our minds to speak theistically *whatever the empirical circumstances might turn out to be*. This, doubtless, would suffice for some, who speak theistically, much as Spinoza spoke monistically, merely to give expression to a particular way of looking at things, or of feeling about them. And it would also suffice for those who make use of the term "God" to cover whatever tendencies towards righteousness and beauty are actually included in the make-up of our world. But it wouldn't suffice for the full-blooded worshipper, who can't help finding our actual world anything but edifying, and its half-formed tendencies towards righteousness and beauty very far from adorable. The religious frame of mind seems, in fact, to be in a quandary ; it seems invincibly determined both to eat its cake and have it. It desires the Divine Existence both to have that inescapable character which can, on modern views, only be found where truth reflects an arbitrary convention, and also the character of "making a real difference" which is only possible where truth doesn't have this merely linguistic basis. We may accordingly deny that modern approaches allow us to remain agnostically poised in regard to God : they force us to come down on the atheistic side. For if God is to satisfy religious claims and needs, He must be a being in every way inescapable, One Whose existence and Whose possession of certain excellences we cannot possibly conceive away. And modern views make it self-evidently absurd (if they don't make it ungrammatical) to speak of such a Being and attribute existence to Him. It was indeed an ill day for Anselm when he hit upon his famous proof. For on that day he not only laid bare something that is of the essence of an adequate religious object, but also something that entails its necessary non-existence.¹

The force of our argument must not, however, be exaggerated. We haven't proved that there aren't beings of all degrees of excellence and greatness, who may deserve attitudes approximating indefinitely to religious reverence. But such beings will at best be instances of valued qualities which we too may come to

¹ Or "non-significance", if this alternative is preferred.

exemplify, though in lesser degree. And not only would it be idolatrous for us to worship them, but it would also be monstrous for them to exact worship, or to care for it. The attitude of such beings to our reverence would necessarily be deprecating: they would prefer co-operative atheists to adoring zealots. And they would probably hide themselves like royal personages from the anthems of their worshippers, and perhaps the fact that there are so few positive signs of their presence is itself a feeble evidence of their real existence. But whether such beings exist or not, they are not divine, and can never satisfy the demands inherent in religious reverence. And the effect of our argument will further be to discredit generally such forms of religion as attach a uniquely sacred meaning to existent things, whether these things be men or acts or institutions or writings.

But there are other frames of mind, to which we shouldn't deny the name "religious", which acquiesce quite readily in the non-existence of their objects. (This non-existence might, in fact, be taken to be the "real meaning" of saying that religious objects and realities are "not of this world".) In such frames of mind we give ourselves over unconditionally and gladly to the task of indefinite approach toward a certain imaginary focus¹ where nothing actually is, and we find this task sufficiently inspiring and satisfying without demanding (absurdly) that there should be something actual at that limit. And the atheistic religious attitude we have mentioned has also undergone reflective elaboration by such philosophers as Fichte and Erigena and Alexander. There is, then, a religious atheism which takes full stock of our arguments, and we may be glad that this is so. For since the religious spirit is one of reverence before things greater than ourselves, we should be gravely impoverished and arrested if this spirit ceased to be operative in our personal and social life. And it would certainly be better that this spirit should survive, with all its fallacious existential trimmings, than that we should cast it forth merely in order to be rid of such irrelevances.

¹ To use a *Kantian comparison*.

VI.—NECESSARY PROPOSITIONS AND ENTAILMENT-STATEMENTS.

BY P. F. STRAWSON.

At the end of his recent excellent paper on entailment, Mr. Körner rejects the view advanced by Professor Moore that "the proposition that p entails q is not itself a necessary proposition; but, if it is true, then the proposition that $p \supset q$ is a necessary proposition".¹ To me, on the contrary, it seems that Professor Moore's distinction between necessary propositions and entailment-statements contains the germ of a more satisfactory account of "entailment" and "necessity" than has yet, as far as I know, been explicitly advocated. Taking that distinction as my starting-point, I shall summarise as follows the position I propose to expound and defend.

(1) Statements descriptive of the use of expressions are contingent. ("Contingent" is to be used as equivalent to "not-(either necessary or self-contradictory)").

(2) Entailment-statements (*i.e.* statements in which the main verb is "entails") are "intensional contingent statements". Other intensional contingent statements are statements in which the main verb is the verb "to be" and the complement of that verb is some expression such as "necessary", "impossible", "consistent", used in the senses "logically necessary", "logically impossible", "logically consistent". Such words as "entails", and, when used in this way, "impossible", "consistent", "necessary", etc., will be referred to as "intensional words".²

¹ Körner, "On Entailment", *Proceedings of the Aristotelian Society*, 1946-1947, p. 161. Cf. also G. E. Moore, "External Relations" in *Philosophical Studies*, p. 302.

² The word "intensional" is chosen because of its recent use in connexion with the question discussed in the final section of this paper, *viz.* the alleged antithesis between an "intensional" and an "extensional" logic. "Modal" and "non-modal" have sometimes done duty in the same connection. Perhaps "modal" is the more generally familiar term in application to some of the words here classified as "intensional". But the ordinary use of "modal" is both too wide and too narrow for the present context; too wide, because modal expressions have a non-intensional as well as an intensional use; too narrow because not all the words here classified as "intensional" are normally called "modal".

(3) In making contingent intensional statements, we *use* the main intensional words, and *mention* the other expressions they contain. These statements are of a higher order than statements in which the expressions mentioned in the contingent intensional statements are used, but not mentioned. They are contingent as any statement about the use of expressions is contingent, but they differ from other statements about the use of expressions in certain ways to be referred to later on. (The fact that an expression is mentioned will be consistently indicated by putting it in quotation-marks. It will be necessary later to distinguish between different kinds of "mention", but not, I think, to introduce a special notation to mark them.)

(4) Some expressions mentioned in some contingent intensional statements are themselves intensional expressions. (Körner's "E²-propositions" are expressed in statements of this kind.¹ But it is necessary to distinguish, as Körner does not distinguish, two classes of E²-statements. It is necessary to distinguish E²-statements mentioning two expressions in which the intensional components are identical from those mentioning two expressions in which the intensional components are not identical. Of these the latter, but not the former, may be held, in Körner's language, to "determine the logical meaning of entailment". The failure to distinguish between them is the foundation, or a part of the foundation, of C. I. Lewis's erroneous claims for an "intensional logic" (see below, last section).

(5) To every true entailment-statement there corresponds a necessary statement.² This correspondence is *not* logical equivalence. It can be described by saying that every true entailment-statement is logically equivalent to another contingent intensional statement which *mentions* a necessary statement or its contradictory. Thus every contingent intensional statement of the form "'p' entails 'q'" is logically equivalent to another contingent intensional statement of the form "'p \supset q' is necessary'; or to a contingent intensional statement of the form "'p. not-q' is impossible'. When an entailment-statement is false, the logically equivalent contingent intensional statement of the form "'p \supset q' is necessary'" fails to mention a necessary

¹ Cf. Körner, *op. cit.*, pp. 145-152.

² I shall often use the word "statement" to refer to what it would be more correct to call, say, a "statement-form" (an expression, containing free variables, which could be turned into a statement by "binding" the variables or substituting appropriate constants). A statement-form is necessary when every statement formed from it in one of these ways is necessary.

statement in spite of the claim to do so. Thus, “‘X is red’ entails ‘X is round’” is false; and “‘X is red \supset X is round” is not necessary. A false entailment-statement is not impossible or self-contradictory, but simply false; just as a true entailment-statement is not necessary, but simply true.

(6) I propose so to use the word “entails” that no necessary statement and no negation of a necessary statement can significantly be said to entail or be entailed by any statement. That is, the function “‘p’ entails ‘q’” cannot take necessary or self-contradictory statements as arguments. The expression “‘p’ entails ‘q’” is to be used to mean “‘p \supset q’ is necessary, and neither ‘p’ nor ‘q’ is either necessary or self-contradictory”; or “‘p . not-q’ is impossible and neither ‘p’ nor ‘q’, nor either of their contradictories, is necessary”.

It will be seen that the outstanding features of these suggestions are the two following: (i) the assertion that entailment-statements are contingent; (ii) the denial that necessary statements (or their contradictories) entail or are entailed by any statement. These conventions are recommended on the general ground that they enable us to clear up some confusions in our use of “entailment” and “necessary” without denying any facts about deductive inference. More specifically, their acceptance has the following advantages: (i) it provides a simple solution to the so-called “paradoxes of implication”; (ii) it points in another way to the truth to which what has been variously called the “linguistic” or the “conventionalist” theory of *a priori* propositions points; (iii) it indicates the source and nature of the mistaken belief that there is an antithesis between “extensional” and “intensional” logic. I shall now attempt to show that these advantages are secured and that no seriously unwelcome consequences are involved.

The “Paradoxes of Implication”.

Nobody now accepts—if indeed anyone ever made—the identification of the relation symbolised by “ \supset ” with the relation which Moore called “entailment”. That is, “ $p \supset q$ ” (i.e. “it is not the case that p and not- q ”) is rejected as an analysis of “‘p’ entails ‘q’”, because it involves such paradoxical consequences as that any false proposition entails any proposition and any true proposition is entailed by any proposition. It is a commonplace that C. I. Lewis’s amendment had consequences scarcely less paradoxical. For if p is impossible (i.e. self-contradictory), it is impossible that p and not- q ; and if q is necessary, not- q is impossible and it is impossible that

p and not- q . That is to say, if “‘ p ’ entails ‘ q ’” means “it is impossible that p and not- q ”, then any necessary proposition is entailed by any proposition and any self-contradictory proposition entails any proposition. On the other hand, Lewis’s definition of entailment (*i.e.* of the relation which holds from “‘ p ” to “‘ q ” whenever “‘ q ” is deducible from “‘ p ”)) obviously commends itself in some respects, and I have, indeed, asserted something very like it in (5) above. Now it is clear that the emphasis laid on the “expression-mentioning” character of the intensional contingent statement by writing, as in (5) above, “‘ p . not- q ’ is impossible” instead of “it is impossible that p and not- q ” does not avoid the paradoxes; but it is equally clear that the addition of the provision of (6) above *does* avoid them. For let us assume that “‘ p_1 ” expresses a contingent, and “‘ q_1 ” a necessary, proposition. Then “‘ p_1 . not- q_1 ’ is impossible because “not- q_1 ” is impossible. But “‘ q_1 ’ is necessary. So, by (6), “‘ p_1 ” does not entail “‘ q_1 ’”. Nor have we avoided the paradoxical assertion that “‘ p_1 ” entails “‘ q_1 ’”, merely to fall into the equally paradoxical assertion that “‘ p_1 ” entails “‘ q_1 ’ is necessary”. For if “‘ q_1 ” is necessary, “‘ q_1 ’ is necessary” is, though true, not necessary, but a contingent intensional statement. Hence “not-(‘ q_1 ’ is necessary)” is, though false, possible. Hence “‘ p_1 . not-(‘ q_1 ’ is necessary)’ is, though false, possible. Hence “‘ p_1 ” does not entail “‘ q_1 ’ is necessary”. Thus, by adopting the views (i) that entailment-statements (and other intensional statements) are non-necessary, and (ii) that no necessary statement or its contradictory can entail or be entailed by any statement, we avoid the paradox that a necessary proposition is entailed by any proposition; and, as will be evident, all the other associated paradoxes.

But, it will be objected, the cure is worse than the disease. The denial that a necessary proposition can entail or be entailed by any proposition (and, therefore, that necessary propositions can be related to *each other* by the entailment-relation) is too high a price to pay for the solution of the paradoxes. For any deductive system consists precisely of necessary propositions related by the relation of entailment. If one necessary proposition cannot entail or be entailed by another, how are such systems possible? This alarm, however, is baseless. Deductive systems do not consist of necessary propositions related by the relation of entailment. They consist of contingent intensional statements to the effect that such-and-such expressions express necessary propositions; and it is these contingent intensional statements that are related to one another

by the relation of entailment. One expression expressing a necessary proposition does not "necessitate" another such expression; but one expression's *membership* of a system of expressions expressing necessary propositions may necessitate another expression's membership of that system. This, as Kneale has hinted,¹ is the real force of Russell's "assertion-sign": it can be understood as an abbreviation for "'...' is necessary"—an intensional mention of the formula which follows. The fact that this rôle is not announced matters little. Any device making it clear that a given expression is a rule of the system would perform the same rôle; and since this is generally obvious, the assertion-sign is in practice superfluous. The mere arrangement of expressions on the page in a systematic way may take the place of any explicit intensional phrase. The point is that it must be made clear that an expression occurs as a member of the system. The expression is necessary in the system; the statement *that* it is necessary in the system is a contingent statement about the system entailed by another contingent statement about the system. Where "P" and "Q" are necessary, "'P' entails 'Q'" has no sense; but "'P' is necessary' entails "'Q' is necessary'" has a perfectly good sense, and may be true. Where "X \supset Y" and "R \supset S" are necessary, "'X \supset Y' entails 'R \supset S'" has no sense; but "'X'" entails "Y'" entails "'R'" entails "S'"' (or "'X \supset Y' is necessary' entails "'R \supset S'" is necessary'"') has a perfectly good sense and may be true.

Expressions and Concepts.

To say that statements of the form "'p' entails 'q'" are contingent makes good sense if statements of this form can be regarded as statements about the *expressions* occurring within the inner quotation-marks. For no one would deny that, in any ordinary sense of the word "contingent", a statement about the use of an expression is contingent. Of course, intensional contingent statements are often (not always) used with a different *object* from that of introducing us to expressions with which we were not previously acquainted—"telling us the meaning of a new word"—which is probably the most familiar object of the use of statements about expressions. Intensional contingent statements are usually made by, and for, those who already know how to use the expressions which the statements

¹ Kneale, "Truths of Logic," *P.A.S.*, 1945-1946, p. 216.

mention. The *objects* of doing so may be various. (To convince ourselves that the making of such statements need not be trivial, even if they are about expressions whose use is known, all we need do, perhaps, is to remember that knowing how to use an expression is not the same as being able to *say* how it is used.) But perhaps the best way to clinch the question is to ask whether any alternative view is possible for anyone who takes it seriously (as I assume everyone does) that, for all p and q , “‘ p ’ entails ‘ q ’” is a statement of a higher order than ““ p ” or than ““ q ” (a “meta-statement”). Taking seriously a difference in order between two statements might be paraphrased as taking seriously the difference in the numbers of quotation-marks they contain. Now since the normal use of quotation-marks is such that a statement which contains a set of quotation-marks and an expression within those quotation-marks, would be said to be about that expression, it might be supposed that anyone who agrees that “‘ p ’ entails ‘ q ’” is a statement of a higher order than ““ p ” or than ““ q ” (i.e. who accepts the above as a proper notation for entailment-statements) would also agree that an entailment-statement is a statement about the *expressions* occurring within the sets of quotation-marks which end and begin immediately before and immediately after the word “entails”. But this would be a mistake; the first agreement is not always accompanied by the second. Some people seem to think, for instance, that in addition to using the word “red” to talk about coloured objects, and to using the expression “‘red’” (as I have just done) to talk about the word “red”, there is a third procedure, *different from either of these, viz.* using the expression “‘red’” to talk about the *concept expressed by the word “red”*. That there is a procedure which is correctly so described I have no doubt; and if (which I doubt very much) it is different from the second procedure, and not merely one form of the second procedure, then clearly there is available to those who admit that entailment-statements are about *statements* an alternative to the view that they are about *expressions*. Mr. Körner, in the article already referred to, writes as if he had such an alternative view in mind. He says that any “entailment-proposition” helps to “determine the logical meaning” of the “proposition” which precedes the word “entails”;¹ or, more exactly, it helps to determine the logical meaning of those *concepts* the designation of which in that part of the statement which precedes the word “entails”

¹ Körner, *op. cit.*, p. 144.

is not repeated in that part of the statement which follows it.¹ But what sense are we to give to the word "proposition" in the context of this phrase, "determining the logical meaning of a proposition"? There is a sense of "proposition" in which it is correct to say that a proposition may be expressed by a sentence; but in this sense it would be more usual to say that a proposition *is* a meaning than that it *has* a meaning; and I think it is plain, therefore, that Mr. Körner, who speaks of the "meaning of a proposition", is not using the word "proposition" in this sense. I think, then, that Mr. Körner must be using the phrase "the meaning of a proposition" as equivalent to something like "the meaning of all sentences having the same meaning as a given sentence".² Mr. Körner's use of the word "determine" also calls for comment. The word sometimes has a future reference and means "to fix precisely what shall be the case"; sometimes it means rather "to ascertain, to record, exactly what is, or has been, the case". Certainly entailment-statements might be "determinant of usage" in either of these ways: in the latter case, their truth or falsity would depend upon past and present usage; in the former, they might be said to be "made true or false" by subsequent usage. But, of course, the matter is not so simple as that. "Determining a usage" is not just a matter of making a record or a forecast. The record is selective, the forecast hortatory. Entailment-statements are used, not simply to describe a usage, but to endorse, or to invite conformity to, the usage they describe. As invitations, they may be acceptable or not; but it is only as descriptions that they are true or not. It appears, then,

¹ Mr. Kneale seems to take a similar view; and on the basis of it asserts that entailment-statements "reflect . . . objective necessities". *Cf. op. cit.*, pp. 232-234.

² The same question arises over Mr. Körner's use of "concept" as over his use of "proposition". He speaks (p. 150) of the adoption of a new entailment-proposition resulting in a change in "the logical meaning of the concept" where it is quite plain that he means to speak of what would normally be called "a change in the conceptual force (meaning) of an expression" or "the use of the same expression to express a different concept"; and, indeed, he speaks in more or less this language himself on the following page. When this confusion is cleared up, it is apparent that Körner's talk of "alternative conceptual systems" (p. 151) is incompatible with his denial that entailment-statements are contingent. For to talk of alternative conceptual systems is to say that, without the world being any different, our way of describing it might have been different. Descriptions of our way of describing the world are therefore contingent. But entailment-propositions are "determinants of a conceptual system", *i.e.* (partial) descriptions of a way of describing the world; and hence contingent.

that Mr. Körner's contention that entailment-propositions "determine the logical meaning of propositions and concepts" is not a genuine alternative to the view that entailment-statements are about the use of expressions; but, with certain reservations, is roughly equivalent to the contention that entailment-statements describe certain features of the use of all expressions having the same use as a given expression. So interpreted (and with the reservations made above regarding "description"), the contention seems to me sound; but it also seems to me as plain as anything that descriptive statements about the usage of expressions are correctly called "contingent" and not, if true, "necessary", or, if false, "self-contradictory". (The reservations make no difference here: it would, for instance, be equally inappropriate to call an *invitation* either "contingent" or "necessary".) Like *all* other contingent intensional statements, and *unlike* many other kinds of descriptive statements about expressions, entailment-statements have the characteristic that the substitution, for any expression mentioned in the statement, of another expression having the same meaning as that expression, yields a statement having the same truth-value as the original statement.¹ This is one of the reasons why people are tempted to say that entailment-statements are necessary.

Another reason why people are tempted to say this, is the use of the same confused "If . . . then . . ." formula to perform the functions both of entailment-statements and of the corresponding necessary statements. Since the "If . . . then . . ." formula has yet other uses as well, it is a very potent source of confusion. I shall give an indication of how this confusion works. Someone may say, "If you drink the whole bottle, you'll feel ill in the morning"; and this statement—

¹ There might seem to be a problem here. Suppose S_1 is an entailment-statement to the effect that " E_1 entails E_2 " and S_2 is an entailment statement to the effect that " E_3 entails E_4 ", where " E_1 " has the same meaning as " E_3 " and " E_2 " has the same meaning as " E_4 ". If entailment-statements are statements about the use of classes of statements "having the same meaning", are we not required to say that S_1 and S_2 , being about the use of the *same* class, have the same *meaning*, in spite of the fact that someone may know that S_1 without knowing that S_2 (*e.g.*, someone may know that "The king is dead" entails "Someone is dead" without knowing that "Le roi est mort" entails "Quelqu'un est mort")? No, we are not of course required to say this, since the given member of the class, by reference to which the class is defined, is different in the two cases: (*e.g.* we are not required to say that "All the men in the same college as John are philosophers" means the same as "All the men in the same college as Robert are philosophers", where Robert and John belong in fact to the same college).

(S1)—exemplifies a well-known method of giving information about the way things happen to go on in the world, about (as we say) causal connections. It has a resemblance to "If your parents have a second child, you will be a sibling" (S2). But obviously S1 is also different from S2; and someone may describe the difference by repeating S2 and saying: "But this statement is logically necessary" (S3). Now S3 is *about* the statement S2, is informative about the use of the expressions occurring in it, and gives, in fact, exactly the same information as would be given by "'Your parents will have a second child' entails 'You will be a sibling'" (S4). Now since in practice people are not often misled into thinking (do not behave as if they thought) that statements like S2 describe the way the world goes as statements like S1 do, it is not often necessary to adopt the way of talking represented by S3 and S4. Instead, *and very, very often*, expressions like S2 (i.e. "If . . . then . . ." expressions) are used to perform the very same informative office that is performed, with more pomp and ceremony, by statements like S3 and S4. So, then; sometimes the expression "If A, then B" *has the very same use* as the expression "If A, then B" is necessary"; and it must be plain that, when this happens, the conjunction "If . . . then . . ." is not operating in the same way in the first expression as it is in the second expression. In the first expression, "If . . . then . . ." has to do the work (the intensional contingent work) which is done by the use of "necessary" and quotation-marks in the second. In the second expression, the work of "If . . . then . . ." is lighter: its function is simply to help in the building up of a *formula*—rendered sometimes "not-(A. not-B)"—which it is the function of the whole expression, *viz.* "'If A, then B' is necessary", to declare inviolable in the usage which is being described or recommended. The inviolability of this formula is the same thing as the deducibility in this usage of statements of the form of B from statements of the form of A. It is to this formula that the expression "necessary" is appropriate. So we have distinguished three uses of the hypothetical ("If . . . then . . .") form. There is its use to give information about the way things go in the non-linguistic world (to tell about causes and motives). There is its use to give information about the way things go in the linguistic world (and to encourage them to go in this way): this is the contingent intensional use, the use of "If . . . then . . .", without specifically intensional words, to perform the function of an entailment-statement. And finally, there is its use in the building up of a formula, the declaration of which

to be "necessary" is equivalent to the assertion of deducibility made by an entailment-statement. It was an insufficiently appreciated part of Russell's achievement to reveal how different was this third use from the other two. Attempts to make it look homely and familiar are misplaced, because they obscure this important difference. The hypothetical statements of our ordinary speech employ "If . . . then . . ." in its first and second uses: to inform about (and to threaten and to bluff about) the non-linguistic world; or to inform about (and to remind of, and to encourage and to invite to) behaviour in linguistic worlds. But "If . . . then . . ." expressions of the third type, and necessary statements generally, though the declaration that they are necessary is informative (or, sometimes, misinformative), do not themselves inform: they are *formulae* to be repeated as an aid to working, verbal exercises for getting in practice for inference, or, occasionally, moves in the game called "constructing a deductive system".

It is relevant at this stage to point out one result which will ensue from the adoption of our conventions for the use of "entails" and "necessary". Since we have rejected necessary statements as possible arguments of the function "'p' entails 'q'", we shall have to deny one set of theorems that are commonly asserted in discussions of "entailment" by logicians. We shall have to reject the statement that "'p' entails 'q'" entails "'p \supset q"'; and this will be merely a special case of the denial that "'p' is necessary" entails "p". Similarly for other intensional words: e.g. we shall have to reject "'p' is impossible" entails 'not-p''. This consequence seems to me a considerable recommendation of our convention. The rejected theorems are not only unwanted in our ordinary thought about deducibility; they seem to me to obscure two central points already mentioned. The first of these is the difference in level, in order, between intensional statements on the one hand, and, on the other, those statements which contain, but do not mention, the expressions mentioned in the intensional statements. The second point obscured by the rejected theorems is that expressions occurring in necessary statements occur in the way hinted at in the previous paragraph: a way that is certainly not "a mention" and is not exactly "a use"; a way which has led some people to say that necessary statements do not express propositions. The adoption of the recommended convention does not involve saying this unless we add the further convention that "a proposition" is to be defined as "a possible argument (or what is expressed by a possible argument) of the function

“‘*p*’ entails ‘*q*’”’. If we agree to characterise the way expressions occur in necessary statements by calling the latter, say, “rule-formulae”¹ (or, when they contain no variables, “rules”), the peculiar appropriateness of the recommended convention is at once apparent; for clearly it would not be significant to say “‘‘*X*’ is a rule-formula’ (*i.e.* “‘*X*’ is necessary”) entails ‘*X*’’. It is plainly not consonant with our ordinary use of “deduce” to say that a rule (or rule-formula) can be *deduced* from the statement that it is a rule (or rule-formula); nor with our ordinary use of “follows from”, to say that a rule (or rule-formula) *follows from* the statement that it is a rule (or rule-formula). On the other hand, it is perfectly consonant with the use of these expressions to say that a statement like “‘*Y*’ is a rule” follows from, is deducible from, is entailed by, a statement like “‘*X*’ is a rule”. Generally, then, the rejection of “‘‘*p*’ is necessary’ entails ‘*p*’” is a double rejection. It is a denial that a statement about the *use* of expressions can entail a statement which is not about the *use* of expressions (*i.e.* specifically, that an intensional statement can entail a non-intensional statement²); and it is a denial that the word “entailment” is correctly used except for a relation between “informative” statements (in the sense of “informative” which includes false, *i.e.* *misinformative*, as well as true, informative statements). Entailment-statements (and contingent intensional statements generally) are informative about language-systems, and may entail one another; we have called them “meta-statements”. Ordinary contingent statements (contingent statements in which all the constants have a use and not a mention) are informative about the non-linguistic world; we may refer to them simply as “statements”. Necessary statements, then, might be called “quasi-statements”, to indicate that they neither *mention* the expressions of which they are composed, nor *use* them to talk about the non-linguistic world, but function in the ways I have hinted at. Now deduction is a means of making the most of our information, of increasing our explicit knowledge. The statement that a given expression is necessary increases, if it is true, our knowledge (of a language-system), but the expression itself does not increase our knowledge

¹ Cf. G. Ryle, “Why are the Calculuses of Logic and Arithmetic Applicable to Reality?” *Aristotelian Society Supp.*, vol. xx, p. 26.

² It would be a mistake to say, generally, that no “mention-statement” can entail a “use-statement”. *E.g.* “‘*p*’ is true” entails “*p*”. But “‘*p*’ is true”, though informative, is not “informative about the use of ‘*p*’”.

of anything. Thus a quasi-statement is never either the premiss or the conclusion of a deductive inference, though the meta-statement that it is necessary may be either or both. Quasi-statements do not entail and are not entailed. Persistent confusion on this point results both from the use of the ambiguous "If . . . then . . ." form, and from the use of modal expressions without the suitably-placed quotation-marks which clarity requires when these expressions are used "intensionally" (though not, of course, when they are used to talk of chances and causes).

Intensional and Extensional Logics.

No entailment-statements are necessary statements. If we adopt this convention, what are we to say of "intensional" logical calculuses like that of C. I. Lewis, in which there occur, as *rules of the system* (i.e. as necessary statements), expressions of the form " p strictly implies q " where "strictly implies" is to be understood, as "entails" is understood, as the converse of "deducible from"? What in general are we to say of the alleged antithesis between an "intensional logic" and an "extensional logic", and the claim that the former is somehow more fundamental than the latter, that it explains the "nature of deducibility" in a sense in which the latter fails to do so? I think it is consistent with the *practice*¹ of most of those who speak of "intensional logical systems" to define an "intensional logic" as a system in which entailment-statements are held to occur as necessary statements or rules of the system, i.e. in which they occur *within* the system and not merely as *descriptive of* the system. I think it is fairly simple to show that the attempt to construct an "intensional system" in this sense is a mistaken attempt; and that, though there is a different sense of "intensional system" in which there can be fairly claimed to be such a system, this sense of the phrase is not compatible with any antithesis between "intensional" and "extensional" logics and is still less compatible with the claim that such a system is the most important as regards deduction, or is, as Lewis puts it, a "canon and critique of deductive inference".²

¹ See, for example, Lewis and Langford, *Symbolic Logic*, esp. ch. viii; Nelson, "Intensional Relations," *MIND*, 1930; Emch, "Implication and Deducibility," *Journal of Symbolic Logic*, 1936.

² Lewis and Langford, *Symbolic Logic*, p. 247.

First it is necessary to notice that certain consequences do *not* follow from the rejection of the view that entailment-statements are, if true, necessary. To begin with, it does *not* follow that there are *no* necessary statements in which the word "entails" and other intensional words occur. More exactly, it does *not* follow that there are no true entailment-statements which *mention* "entails" and other intensional expressions. On the contrary, there of course *are* true contingent entailment-statements of this kind. They will be statements about the relations of use between expressions like "deducible from", "impossible", "necessary", "entails", "consistent" and so on. An instance of such a statement will be "*"p*" entails "*q*"" entails "*p*" is consistent with "*q*""; another will be its equivalent, "*"p*" entails "*q*" \supset "*p*" is consistent with "*q*"" is necessary". The set of such statements will be a closed system in the sense that the only expressions *directly* mentioned¹ in these statements will be intensional expressions. Our conventions allow for the occurrence of such a set of true entailment-statements. Let us call it the "Closed Intensional Set". This set is descriptive of the language-system within which such expressions as "entails" are used, and *within which* such expressions as "*p*" entails "*q*" \supset "*p*" is consistent with "*q*" (or "it is not the case that '*p*' entails '*q*' and '*p*' is not consistent with '*q*'") are necessary. If the claim for an "intensional system" is the claim that there exists the Closed Intensional Set, it is quite plain that (a) the claim must be granted, and (b) the "intensional system" (in this sense) is certainly not a "canon and critique of deductive inference", but is of rather minor importance for inference, since we rarely wish to make inferences of which the validity depends upon the meaning of intensional words. It is also plain that there is no antithesis between such a set of statements and any other set of entailment-statements mentioning different expressions.

Secondly, it does *not* follow, from the rejection of the view that entailment-statements are, if true, necessary, that there are no true entailment-statements to the effect that one entailment-statement entails another entailment-statement (or to the effect that one statement that a certain expression is necessary entails another statement that a certain other expression is necessary). On the contrary, as has been stressed, it is because there are entailment-statements of this kind that deductive systems are possible. It is, however, extremely important to distinguish

¹ A mention of "*p* and not-*q*" is impossible" is a direct mention of "impossible" and an indirect mention of "and" and "not".

between entailment-statements of this kind and entailment-statements of the kind just discussed, namely those that belong to the Closed Intensional Set. I propose to bring out the difference by means of an analogy. Let us call the expressions "greater than" and "smaller than" both "magnitude-expressions". Let us call statements in which these phrases are used "magnitude-statements". A statement which *uses* the magnitude-expression "greater than", uses it once only and uses no other magnitude-expression, will be called a Type 1 Magnitude-Statement. An entailment-statement which is *about* magnitude-expressions will be said to belong to the "Magnitude Set" of entailment-statements. Thus "'A is greater than B' entails 'B is smaller than A'" belongs to the Magnitude Set and mentions the two magnitude-expressions. Now there occur true entailment-statements, which are statements to the effect that one magnitude-statement entails another magnitude-statement, but which do not belong to the Magnitude Set. For example, "'Something red is greater than something square' entails 'Something coloured is greater than something rectangular'" is such a true entailment-statement, where both the magnitude-statements concerned are Type 1 Magnitude Statements. It is obvious that, where the magnitude-statements concerned are of the same type, the truth of the entailment-statement is independent of the meaning of the magnitude-expressions concerned, *i.e.* independent of the truth of any entailment-statement belonging to the Magnitude Set. We may express this by saying that the entailment-statement concerned is not a "significant mention" of the magnitude-expression; the expressions "significantly mentioned" in our example are "red", "coloured", etc. The application of the analogy is, I hope, obvious. Entailment-statements to the effect that one entailment-statement entails another entailment-statement are not a significant mention of the word "entails", and their truth is independent of the truth of any member of the Intensional Set; and entailment-statements to the effect that a statement that one expression is necessary entails a statement that another expression is necessary are not a significant mention of "necessary", and *their* truth is independent of the truth of any member of the Closed Intensional Set. (The most important of the expressions which *are* significantly mentioned in such statements are the quantifiers, and the logical constants of the propositional calculus.)

Now one possible cause of the large claims for an "intensional logic" is a confusion between entailment-statements which

belong to the Closed Intensional Set and entailment-statements to the effect that one intensional statement entails another intensional statement of *the same type*. The distinction, we have just seen, is clear and important: the former, but not the latter, represent a significant mention of the intensional words. If the distinction is not drawn, then it will be imagined that the pillars of every deductive system are the statements of a system which "investigates deducibility" (*i.e.* the statements of the Closed Intensional Set, which significantly mention intensional words); and thus the claim arises that the intensional system is the "canon and critique of deductive inference". That the mistake is made is shown by Mr. Körner's making it, in that section of his article which deals with "E¹- and E²-propositions in mathematics".¹

The confusion just described is a mild and limited form of what we might describe as the "intensionalist error": namely, the error of thinking that whenever it is a fact that one statement is deducible from another, this is *because of* the meaning of "deducible" or "entails" or some other intensional word; or, in other words, the error of thinking that all true entailment-statements state facts about the "nature of entailment". (Perhaps the error is rarely committed in this extreme form: it is certainly often committed as regards entailment-statements mentioning such expressions as "or", "if", "some", etc.). It is, of course, the case that whenever one expression is deducible from another, this is "because of the meaning of" (the result of the usage of) *some* expression occurring in one of the statements and *some* expression occurring in the other. No one, I think, ever intended to deny the inter-dependence of meaning and deducibility in this sense (except, perhaps, Carnap at one

¹ Körner, *op. cit.*, pp. 151-152, especially the following:—

"The difference between various mathematical systems . . . lies in their postulates and theorems which determine the logical meaning of their concepts; while the validity of the E²-propositions which assert that the postulates entail the theorems does not depend on the logical meaning of these concepts. These E²-propositions determine the logical meaning of the entailment relation".

It is quite true that the validity of these E²-propositions does not depend on the meaning of the expressions peculiar to the system concerned. It is quite false that their validity depends upon the meaning of intensional expressions like "entailment". Their validity depends upon the meaning of some expressions (*e.g.* "all", "or", "not") which are common to different mathematical systems, but which are not the subject of direct mention in those statements which belong to the Closed Intensional Set, or, in Körner's language, "determine the logical meaning of the entailment relation".

time, and, perhaps, Wittgenstein at another); although some "intensionalists" have spoken as if it *were* denied and as if it had been claimed that the deducibility of one statement from another depended in some cases solely upon the truth-values of the statements concerned. If the intensionalist thesis were merely, then, that the truth of any entailment-statement depended upon the meaning, the use, of some expressions mentioned in each such statement, it would be undeniable; but, of course, if to call a statement "intensional" is to mean merely this, then there is no sense in talking of an "antithesis" between an intensional and any other kind of logic, and any necessary statement whatever (e.g., "it is not the case that anything is red and not coloured", or any necessary statement of the propositional calculus) is necessary "in the intensional system", which cannot, therefore, claim to be especially concerned with the relation of deducibility.

It remains to show that there is no such thing as an "intensional system" in the sense defined at the beginning of this section. (The connection between the belief in such a system and the mistakes just discussed will, I think, be obvious.) If there is an "intensional system" in this sense, then there are a lot of true statements of the form

"‘‘*p*’’ entails ‘‘*q*’’ is necessary”

differing from one another in respect of the expressions occurring within the inside double quotation-marks; and each of these statements is "informative about deducibility". But information about the specific deducibility of a particular expression "p" (and others having the same meaning) from a particular expression "q" (and others having the same meaning) is already given in the corresponding particular statement of the form "‘‘*p*’’ entails ‘‘*q*’’". The *additional* information about deducibility given by "‘‘*p*’’ entails ‘‘*q*’’ is necessary", then, is *not* different for different particular values of "p" and "q". But statements of this form differ from one another *only* in respect of the values of "p" and "q". Therefore it is *not* the case that there is a set of true and informative statements about deducibility of the form "‘‘*p*’’ entails ‘‘*q*’’ is necessary" (wherever, or in some cases where, "‘‘*p*’’ entails, ‘‘*q*’’ is true), and it is accordingly *not* the case that statements of the form "‘‘*p*’’ entails ‘‘*q*’’" are ever laws or rules of an "intensional system". The fact that C. I. Lewis was able to obtain, e.g., *both* the expression "*p*. *p* \supset *q* : \supset . *q*" and the expression "*p*. *p* \supset *q* : strictly implies . *q*" as necessary propositions or

laws of the system, instead of being claimed as a triumph for strict implication,¹ should have suggested that something was wrong. If to say that " $p \cdot p \supset q : \supset . q$ " is a law of the system is equivalent to saying that " q " is deducible from " $p \cdot p \supset q$ ", then what additional information about the deducibility of " q " from " $p \cdot p \supset q$ " is given by saying that " $p \cdot p \supset q$ " strictly implies ' q '?" is a law of the system? If, on the other hand, the statement " $p \cdot p \supset q$ " strictly implies " q " is a law of the system" means merely that " $p \cdot p \supset q$ " entails " q ", then what on earth is the significance of calling " $p \cdot p \supset q : \supset . q$ " a necessary proposition or a law? This is not in any case an open alternative, since " $p \supset q$ is necessary" is to be rendered, in Lewis's terms, as "it is impossible that not- $(p \supset q)$ ", which is to be read as " p . not- q is impossible", which is equivalent to " p entails ' q '" or " q is deducible from ' p '". Briefly, for any expression of the form " $p \supset q$ " which Lewis obtains as a law in his system, he also obtains "it is impossible that not- $(p \supset q)$ " as a law. Now not only is it absolutely plain that, from the point of view of information about deducibility, one of these expressions is redundant. It is also plain that Lewis is committed to the possibility of obtaining an infinite series of such laws. And indeed it is only by the adoption of the convention here recommended (*viz.* that intensional statements are contingent), that such an infinite series is avoidable. For if " p is necessary" is necessary whenever it is true, then " p is necessary" is necessary" is necessary, and so on. There seems to be no virtue in such an indefinite multiplication of orders of necessity.

¹ Lewis and Langford, *op. cit.*, pp. 245-246.

VII.—MENTAL ACTS.

BY A. C. EWING.

Philonous. Surely there are such things as philosophers have called mental acts. Surely we know, infer, believe, doubt, will, decide, etc. ? The *cogito, ergo sum* of Descartes already proves the existence of at least some of these acts, and we are aware by introspection of the others. For instance I decided to write this article while listening to a discussion on Mental Acts at the joint session of the Aristotelian Society and Mind Association of 1947. I knew perfectly well at the time that I made the decision then, yet at the time I took no external action—that only began some time later—I knew it by introspection. And I certainly know whether I am believing or doubting a proposition that is proposed to me, and I know it before I have taken any action on the belief, so that we cannot reduce believing to a kind of overt action. Introspection, so far from being uncertain, is the most certain of all avenues of knowledge. It is in fact infallible, and we discover the existence of mental acts by introspection.

Empiricus. But what are these mental acts ? Can you give any description of what it is like to know, decide, etc. ? You can mention the occasions and effects of your knowledge and decisions, you may (unless you are a behaviourist) perhaps tell me of the images which accompany them, you can at any rate repeat the subvocal language used by yourself in connexion with them, but you cannot tell me what they are like in themselves. You may say they are indefinable, but does not your inability to describe any one of them raise suspicions ? We are justified in assuming indefinable entities only if we have a clear experience of what they are like, since otherwise the words just have no meaning, and have you this experience ? You may claim to know them by introspection, but many people who are quite as good at introspection as you—in fact many expert psychologists, not to say philosophers—have denied this introspective knowledge of them. When I look for them and try to examine them, they seem to vanish into a diaphanous nothing. Introspection is up to a point clear and certain. I can know that I have a pain, I can know that I have certain sensations (or at least sense-data), but when I look for an act of knowing I cannot find it. Now you may say I am mistaken, you may protest that they are there although I cannot find them, but should not the mere fact that

many people cannot find them when they introspect cast doubt on their existence ? If there are such things they must be extremely common, one for every time we know, etc., and, if so, surely everybody would, at least sometimes, be able to detect them ? There is no doubt that pains occur ; if acts of knowing were possible objects of introspection like pains, there should be no doubt that they also occur. Such common and fundamental elements of mental life as these are alleged to be surely could not be missed ? Further, an examination of language reveals that psychological terms such as 'knowing' or 'believing' or 'desiring' do not stand for single acts or events of any sort but for dispositional states. We can say truly that somebody knows, believes or desires something even if the person happens to be in a state of dreamless sleep when we use the words. Therefore, though these words no doubt stand for something, they do not stand for mental acts, and the fact that we can sometimes know that we know something does not prove that there are knowing acts.

This dialogue expresses one of the fundamental differences of modern philosophy. The conflict has, I think, no single source, but is bound up with a whole mass of complications and misunderstandings, no doubt too intricate to be ferreted out adequately in a single article. Yet without solving it we can have no tolerable theory of mind. It is therefore very disappointing that relatively so little has been done by philosophers to come to terms on the subject. The wide divergencies between philosophers and the incredible assertions that were made on one (the anti-act) side in the discussion to which I have referred while the other remained largely unexpressed, though no doubt accepted by very many, have prompted me to make an effort to come to the rescue.

Now, I think, Philonous had better admit that our psychological terms are not, as they stand, usually just names for mental acts. To say that I know or believe or desire something is not just to say that I perform an act of knowing, etc. It is rather to say that I have a certain disposition. The same applies to words for emotions—being angry is not a single mental act—only here what the words referred to would be called not a "disposition" but a "state of mind". This is because to say that a man is angry usually implies that there are actual qualities of angry feeling, or at least irritability, in his experience throughout the time when he is described as being angry—we cannot be angry in a dreamless sleep—while to say that a man knows, believes or desires something does not imply that he is conscious of it in any way all the time during which he can correctly be described as

knowing, believing, or desiring it. There are, however, some psychological terms which do seem to stand, at least sometimes, for mental acts. Thus the word "see" in the non-physical sense of "see" stands for a definite experience which would seem to be detectable introspectively when we say after a period of puzzlement "Ah, this is clear to me now". Whether this experience is best described as an act is another question, but there does seem to be a definite experience which we can get hold of there. This suggests that Philonous might equate the mental act primarily concerned in knowledge not with what is meant by "knowing" but with what is meant by "coming to know". To "see" in the sense in which I have just used it is to come to know the relation of something, *e.g.* the relation between a premiss and a conclusion. And he might contend that also words like *decide*, *choose*, *will*, stand for mental acts and not dispositions. Other words for which he might plausibly advance this claim are *perceive*, *recollect*, and *entertain* (in the rather technical sense of entertaining propositions). *Remember* and *infer* seem to be used in both ways. But even if there were no single words which stood for mental acts, it would not oblige us to deny mental acts or dismiss the term as meaningless. It may well be the case that a word implies or presupposes something for which it does not itself stand. An example is provided by the use of colour words. There is no case of a single word standing for a single determinate shade of colour, yet the use of our ordinary colour words such as red, yellow, etc., presupposes such shades. The terms have meaning only as covering a range of such shades. If nothing had a determinate shade of yellow at any moment, nothing would ever be yellow at all. Similarly, Philonous might say, "know" does not stand for any single mental act, but if there were no mental acts of a certain kind we could never "know". And logically, it would seem, the use of a dispositional word must presuppose awareness of certain occurrents or events. For a "disposition" seems to signify simply the tendency for certain events to occur: it is nothing actual.

But Empiricus may advance a different interpretation of the disposition words! he may say they stand not for a disposition to perform certain mental acts but for a disposition to overt action. Thus "believing" has been regarded as a disposition to act as if what we are said to believe were true. And those like him have used as an argument for their view the fact that, if a person says that he believes A but acts as if he did not, we should be inclined to say that he did not believe A. It might be retorted, however, that this is only because we regard his act as external

evidence for his beliefs and so infer the latter from the former. With this reply I should be inclined to agree. The inference we can indeed easily see to be a somewhat precarious one in view of human inconsistency and the fact that we are liable to be swayed by various desires to do what we believe to be imprudent, thus acting as if A were not true while believing A. But if action as though what we are said to believe were true were what is *meant* by "believing", there would be nothing precarious about the inference in question. Empiricus might try to meet this objection by introducing the word "tendency" and defining belief in terms of a tendency to act in a certain way. But suppose I act as if A were not true, although I believe A to be true. Suppose, for instance, on the authority of a doctor I believe that drink is very bad for my health and yet go on drinking. In that case it might well still be admitted that I have a tendency to abstain from drink, since if I were not otherwise attracted by it I certainly should abstain in order to save my health. I therefore have a tendency to act as if A (the proposition that drink is bad for my health) were true. But I have also in the supposed case a tendency to act as if it were false, for I go on drinking. Therefore I by definition both believe and disbelieve that drink is bad for my health, and since I act on the whole as if it were not, the disbelief may be said to be more marked than the belief. Yet there is surely no contradiction in saying that somebody believed all the time that drink was bad for him and never disbelieved it at all, yet went on drinking. Another difficulty is that it is hard to see what can be meant by acting as if A were B except *acting as if I believed A to be B*, in which case the definition becomes circular. The fact that A is B will not affect the actions in question unless I believe A to be B, though it may affect their results. The fact that a certain medicine will cure a certain disease will not affect my actions when I get the disease unless I first believe that it will or at least may cure it, though it will, of course, affect my success in attaining my object if I do take the medicine in question. Again, I often believe propositions on which I never have occasion to act, *e.g.* most of the information I obtain by reading newspapers and history books. In order to meet this objection belief is defined in terms of hypothetical action. But here Philonous has a strong point—Surely I can know directly (sometimes at least) that I believe something, yet I never know directly (though I may infer) that I should, if something happened which has not happened, do some particular thing. Hypothetical propositions about contingent future facts (at least apart from the sphere of "psychical research") are

never known in this direct way. Empiricus may possibly then reply that at least I act on the belief in one way, namely, I formulate it in words at least to myself when I believe it; but this account seems to involve him in a vicious circle, for merely saying something is compatible with not believing it and we have to add "saying it in the belief that it is true".

Similarly, he will commit a vicious circle if he includes under the actions which constitute belief drawing inferences from the proposition believed, for to infer is not merely to pronounce them, but to pronounce them believing or knowing them to be true. The same criticisms may of course be brought against the parallel account of knowing: in fact, philosophers have usually not spoken of "acts of belief" but of "acts of knowing". I do not myself, however, think that there is any psychological difference between subjectively certain belief without a shadow of doubt and knowledge. The difference is that the terms *knowledge* and *know* have both a psychological and a logical meaning at once. They connote not only that we are subjectively certain but that the certainty is justified, and it is the presence of this second, non-psychological element which constitutes the difference between them and *subjectively certain belief*. But, if there are cognitive acts which yield certain truths, there are surely also cognitive acts which involve the possibility and even the actuality of error. So I shall continue to discuss the matter in terms of believing, though philosophers have not usually spoken of acts of belief. They have spoken of "acts of judgement", which covers not only knowledge but also uncertainty and error, and they might just as well have said "acts of believing". Both with believing and knowing, we must clearly reject any account of them purely in terms of action or hypothetical action.

It is more plausible to say that *part* of what we mean by "believing" is acting as if *p* were true. Indeed, in some cases it seems to be the major part. Thus I can say without hesitation that you believe that the floor will not suddenly explode beneath you, though you have probably never contemplated this eventuality at all, and therefore cannot in any case have performed a mental act of believing that it will not happen. Since there is consequently no mental act or psychological event of any sort directly involved, it would indeed seem plausible in such cases to say that all that was meant was that you acted as if the floor would explode, *e.g.* remained sitting unconcernedly and did not rush out of the house. I think, however, something more is involved in the statement than this. Part of the meaning, I

think, is here to be found in hypothetical *mental* processes. In saying you believe this I mean to say not only that you act externally as if you believed it, but also that you (a) would have an actual conscious mental belief in its truth if the question were proposed to you, (b) would be surprised if something happened incompatible with what I have said you believed. We fortunately have the capacity of acting as if we possessed a number of beliefs which we do not consciously possess, and with most of such beliefs we should, if questioned, have a conscious belief in their truth. In the relatively rare cases where a question was met by a denial of the belief, it would generally be said that the person who uttered the denial did not hold the belief or had ceased to hold it; but there might be people who (without accusing him of lying) still said that he believed the proposition in question if he went on acting as if he did. But they would in that case probably preface the word believe by "really", thus implying that they are using "believe" in not quite the ordinary sense. Take another case: F. C. Schiller told a story of an orthodox Christian who was asked what he thought would happen to him when he died and answered, "I suppose I shall enter into a state of eternal bliss, but I wish you would not talk of such depressing topics". Did he or did he not believe that he would enter into a state of eternal bliss? Most people (provided they trusted his veracity) would say he "believed it in a way" but did not "really believe it". But there are two ways of interpreting this. It may be said that the notion of believing includes acting and feeling in a way which would be rational if the belief were well-founded, or it may be said that it does not include this but that such behaviour and feelings are evidence for the belief being present, though not part of what is meant by "belief". In the latter case we may say that, though the belief must be present in some degree for the man introspectively to discover it, it is evidently not present in full degree if his actions or feelings are flagrantly inconsistent with it so that we are justified in that case in saying he does not *really* believe it. Obviously we must not in either case press this too far; men are very liable to be irrational, and it would be quite ridiculous to say a person did not believe *p* because he did not draw and apply in his actions all the inferences which a perfectly rational being would draw from *p*. It is, however, only a verbal dispute in which of these two ways "believe" is to be used; the important point is that "belief", even if it does include in its meaning a reference to outward behaviour, includes something else besides. That seems quite obvious, and it is at least as obvious with "knowing", yet if

this is admitted it does not necessarily follow that this something is a mental act or acts.

But let our friend Empiricus speak for himself. If he is sensible, he will admit the presence of something that can be introspectively discovered, but he will deny that it is a mental act. Believing itself, he will continue to insist, is a dispositional term, and similarly knowing, and on this linguistic point he is certainly right ; but he may admit that events sometimes occur in connexion with believing and knowing which cannot be observed by the ordinary physical senses, and can normally only be observed by the person who is said to believe or know and not by anybody else. (I say "normally" only to avoid implying a denial of the occurrence of telepathy.) Yes, he will say, when I have a belief I sometimes do not observe anything at all, but sometimes at the time when I acquire the belief I observe the occurrence of verbal or other imagery. But what of this ? Images are admittedly no more like mental acts than are sense-data. But surely, Philonous asks, the belief requires not merely imagery but assent to what is conveyed by the imagery ? And he can support his argument by pointing out that I may, confronted with the same verbal imagery, either believe, disbelieve, or merely entertain the proposition in question. Empiricus may then go a step further : he may admit the occurrence of belief-feelings. It would be in the highest degree paradoxical and un-empirical even for an "empiricist" to deny that we do have such feelings, however they may be analysed ; and why should he ? In regard to the same imagery, he can say, I may have a feeling of conviction or its opposite or a feeling of doubt or no feeling at all. In the first case I can be said to believe, in the second case to disbelieve, in the third to doubt, and in the fourth merely to entertain the proposition. If you care to call this feeling a mental act you are at liberty to do so, but there is no more point in doing this than in applying the term to any other feeling. A throb of pain would not be called a mental act by anyone, nor would a throb of emotion. And feelings of conviction or doubt are only emotions, a point which is usually overlooked because they are in most (though not in all) cases such very mild emotions. This is in fact too simple an account, but the complexities which come now do not necessarily weaken the case of Empiricus. It may be pointed out that there is not always a distinctive feeling in cases of belief or disbelief ; but where the feeling is not present, he may reply that "belief" or "disbelief" is being used in a hypothetical sense as meaning that the person will, if certain circumstances arise, act in a certain way or have a feeling of a certain kind or

both, the feeling being "conviction" if the belief is challenged and "surprise" if it prove false. I do not ordinarily have an introspectively discernible belief-feeling when I am told some particular unimportant fact by a friend or do a simple sum in arithmetic, but in both cases there would be more or less surprise if the belief proved false, and other circumstances might produce a feeling of conviction, if e.g. the belief was unreasonably challenged by someone. If both criteria, the criterion of action and the criterion of ability to evoke surprise or conviction, are subsequently satisfied, we shall assert without hesitation that belief is present; if only one is, some people will prefer to make this assertion, others not, and some will hesitate, but the difference between these parties will merely be the verbal one as to how much they include under the term "belief". But when we have granted imagery and feeling, what else is there to introspect? Is there anything at all? There are cases of belief, all must admit, where we cannot detect a distinct belief feeling, and that is a reason against just identifying belief with the feeling; but even if we admitted mental acts, since belief is a dispositional term, we could not just identify it with the mental acts. We should in any case have to define it in terms of *hypothetical* mental acts. And, where we cannot detect a belief-feeling, can we detect a mental act either? Can we in these cases detect anything at all in our mental process but imagery? Further, even if we suppose that there is a mental act present in all cases, is that anything but supposing that there is always present some peculiar kind of feeling like the feeling of which we are aware in some cases of belief, only in a less noticeable form? What could mental acts be but some kind of feeling?

To this it may be replied that, even if called feelings, cognitive acts are such peculiar kinds of feelings that at any rate they deserve to be put in a separate class distinct from all other feelings and that the difference is indeed so great that it is misleading to suggest they are as like headaches and emotions of anger as is suggested by calling them by the same name, feelings. For they are concerned with truth and falsity in a way in which no other feelings are, and their essential nature lies not in what they feel like, but in a unique kind of relation to a proposition. Headaches and emotions of anger do not apprehend truth, assent to propositions, or commit errors. There is an empirically given difference in nature of a kind which justifies the traditional division of mental process into the three sides, cognitive, conative and affective, and makes it most inappropriate to lump them all together as feelings. Further, though a feeling of conviction may

accompany believing or knowing, can it be identified with the cognitive act of knowing or coming to believe? The feeling may be so weak as to be almost or quite indiscernible in many cases where knowledge is regarded as absolutely certain, and it seems that it may be present very intensely (as in cases of nitrous oxide intoxication) where nothing is believed. And we are aware that we know or believe something in a great many cases where we do not detect anything at all in the way of a belief feeling or emotion of conviction. As I have said, I certainly am not aware of such a feeling each time I am told an unimportant fact by someone, yet in the absence of any special reason for doing otherwise I believe what I am told about matters of fact and know directly at the time that I believe it. In doing sums in arithmetic which give not merely belief but knowledge I do not usually have any detectable feeling of conviction at all, yet I am aware at the time that I know. This awareness cannot be reduced merely to the presence of imagery, for two reasons at least: (1) the same imagery may be accompanied by knowledge, belief, disbelief, doubt, and suspension of judgement; therefore imagery cannot explain the distinction between these mental states; (2) in order to judge, believe, or know we require not only images but an awareness of what the images mean. This awareness of meaning may be interpreted to a certain extent in terms of other images, but we cannot go on with this to the end, and there are very many cases where we have none but verbal images and could not have any others that would serve our purpose at all. Nor can meaning be interpreted in terms of imagery together with action, at least unless we mean by "action" the mental action of thinking in certain ways, which itself would involve mental cognitive "acts". We cannot reduce this thinking to merely talking to ourselves, whether physically or mentally, for the same difficulty will then again arise of explaining in such terms what is meant by saying that the words we use in this speech have a meaning. If we open a book on philosophy at almost any place, we shall find a sentence which is such that its meaning cannot be expressed in any imagery that does not consist of words or other symbols unlike in character to what they represent (since it concerns what cannot be pictured in terms of the senses), and which is also such that belief in its truth could not be connected directly with any resultant physical action except the action of repeating the words to a questioner. If it is then said that the meaning can be translated in terms of action by bringing under the latter head the making of the inferences we should make if we believed it true, this presupposes a mental process of thinking

not reducible to either outward physical action or hypothetical action or imagery. For if we say that the making of the inferences just consists in the using of words, we have provided no means of distinguishing between nonsense-syllables and words which have a meaning not expressible in terms of imagery. There are philosophers who would welcome the conclusion that all metaphysics is nonsense, but their own philosophical books or discourses would likewise have to be regarded as nonsense, and about this their enthusiasm would not be so great. Their philosophical sentences are no more capable than the sentences of metaphysicians of being interpreted solely in terms of images or outward actions or both. Nor are the sentences which cannot be expressed by non-symbolic images confined to philosophy—they are ubiquitous in mathematics except where we are dealing with groups small enough for us to have in our mind all at once, they are almost ubiquitous in economics and politics, and very abundant in physical science and history. Many attempts have been made to analyse meaning in the way indicated, but the examples given almost always concern sensible objects, and the extraordinarily difficult task of dealing in this way with the other kind of propositions in detail has hardly been attempted, as far as I know. (I think myself indeed that the attempt breaks down even as regards sensible objects, because all propositions about such objects involve the use of general terms and I do not see how the meaning of general terms can be adequately handled in that fashion.) So we have to admit besides images an awareness of their meaning which cannot itself be analysed in terms of images. If we care to call this awareness of meaning a feeling we can do so, but it is an odd kind of feeling !

At this point we had perhaps better consider an attack made on mental acts from another quarter, namely by certain "idealists". They do not try to explain away mental acts in terms of imagery and physical behaviour, but they do try to explain them away in terms of "ideas". But it seems to me that an idea as distinct from an image—and they do make that distinction—is not an existent entity discoverable in introspection. Images I know and experiences of thinking I know, but not any third kind of entity, ideas distinct from either of these. Suppose I think of something that cannot be adequately imaged. Firstly, I have the thinking as a mental process; and secondly, I have the image of certain words or whatever other image it is that I use for the purpose; but I cannot discover any other entity by introspection. We may speak of the idea as "the image together with its meaning" or "the image used to mean

something", but this does not introduce a fresh entity but merely consists in viewing the image in a certain relation. An allied tendency with certain idealists like Bosanquet is to include in the self the objects of its knowledge and indeed everything in which it is interested, so that the self has no fixed boundaries and no specific subject-matter that could not also belong to the not-self on another occasion when the self happened not to be interested in it. For mental acts and processes they then substitute the "ideal self-development" of these objects. This seems to me to be talking in metaphors: there are cases where the welfare of another person may become as important to a man as if that person were an essential part of himself, but that does not make the person actually a part of himself; and the object does not alter and develop through my coming to know it better (except as regards the relational characteristic of being better known by me). My feelings, thoughts, etc., are not their objects, and do not include their objects. It seems to me that idealism carried to the point of refusing to recognise the distinction between the mental attitudes towards A and A itself can only make nonsense of any account we give of the world of experience.

To return to the main line of argument, we may sum up the trend of this article so far in dialogue form.

Empiricus. Knowing, believing, etc., do not stand for mental acts because the words may be correctly used of persons who are at the time admittedly not even conscious of what they are said to know or believe.

Philonous. This is only because they, like many other psychological terms, refer to dispositions, but there could not be the disposition if there were not sometimes acts. A mental disposition just means that the person to whom it is ascribed, though he may not be mentally active in the way referred to at the moment, will under certain conditions be mentally active in this way. As a matter of fact there are psychological terms such as "see" in the non-physical sense of "see" which refer directly not to mental dispositions but to mental acts.

Emp. I admit that dispositional terms such as belief involve a reference to action, but the action to which they refer is the physical action a man will undertake if he holds a certain belief.

Phil. This is a vicious circle: the action is only undertaken because the man holds the belief in question and therefore presupposes a belief distinct from itself to account for its occurrence.

Emp. Would you really admit that a person held a belief if it were the case that he would under no circumstances act as if he held the belief? And do you not on the other hand constantly

ascribe beliefs to people on the strength of their outward actions even if it is highly doubtful whether they have ever formulated to themselves and given assent to the beliefs in question? If so, does it not follow that the chief part of what is meant by ascribing a belief to somebody is that he behaves in a certain manner?

Phil. I admit that, if a person does not act in accord with a belief, I may have doubts as to whether he holds the belief, but this is only because a man's actions are indirect evidence for his beliefs; and this evidence must not be pressed too far, since we are all liable to inconsistency. And, in the cases where I ascribe a belief to somebody without committing myself to the view that he has ever consciously accepted the belief, what I mean may be explained not in terms of hypothetical outward behaviour but in terms of hypothetical mental acts of judgement.

Emp. But I am not committed to analysing belief wholly in terms of outward action. I am quite prepared to admit imagery and, where a person consciously believes something, he no doubt has certain images, at least of a verbal kind.

Phil. The belief cannot possibly be identified with the images. For with the very same images I can either believe, disbelieve, doubt, or suspend judgement. Further, the images can only function in my thought if I am aware of what they mean, and we cannot reduce this awareness of meaning to other images.

Emp. On second thoughts I find there is something else which I can sometimes detect introspectively besides these images. I can also detect a feeling of conviction, a kind of (usually mild) emotion, in some cases of belief. But this is certainly not detectable in all cases of belief, and it is certainly not appropriately called a mental act any more than are other emotions.

Phil. No doubt these feelings of conviction sometimes accompany belief, but besides these there is an "act of judgement" (for some obscure reason philosophers do not speak of "acts of belief"). "Belief" stands normally and primarily for a disposition to perform these mental acts. That it does not stand for the feeling of conviction is shown by the fact that in cases where we believe something with the greatest certainty the feeling of conviction may be extremely weak as a feeling or non-existent.

Emp. I do not claim to analyse "belief" merely in terms of feelings of conviction but in terms of these plus outward action plus imagery. If we take only one of these of course the account given is deficient, but the three supplement each other's deficiencies.

Phil. They cannot help each other out in this way. For (1) outward action is insufficient because we often do not act on

a belief. Therefore the belief will on the behaviourist view have to be defined in terms of hypothetical action. But I can know directly whether I believe something and cannot know directly my hypothetical future actions. Now imagery will not help here because the same imagery may accompany both belief and doubt, therefore I still could not know directly whether I believed. Nor can feelings of conviction, since I can know directly that I believe something in many cases where I am not aware of any feeling of conviction whatever. Even if it is possible that the feelings are really there and have been overlooked by us, their presence will not explain our knowledge that we believe, since we at any rate do not know them to be there. (2) To say that believing p means acting as if p were true is to commit a vicious circle, for to act as if p were true can only mean acting as if we believed that p were true. It cannot mean acting as if we had images of p , which is quite compatible with our disbelief in p , and feelings of conviction will not help, since we have seen that we may believe without these feelings being there, or at least that we can know we believe without knowing the feelings to be there.

Emp. In cases of belief where there is no feeling of conviction, I do not think myself that I can detect anything relevant at all except imagery. But have it your own way. Suppose there is something else which you please to call a mental act. Would it not be just another feeling? If so, is anything gained by positing it, and if it does occur does it conflict with my account on principle? Would it not merely show at the most that I had given an inadequate analysis of the feelings empirically present, as might a man who, *e.g.*, ignored the "goosefleshy" sensation in giving an analysis of fear? What would it be like to discover a mental act that was not a feeling at all?

Phil. If an act of judging or knowing is a feeling, it is a highly peculiar kind of feeling. For, look, it apprehends truth and includes awareness not merely of images but of what the images mean. An act of judging is thus a great deal more than a mere "feeling", if the term "feeling" is to have any definite meaning at all. It is most misleading to use the word feeling of the acts since that suggests they are something like a headache or an emotion of anger, while they are generically different from any such things. Of course you can, if you like, lump together all mental occurrences under the heading feeling, but you will not gain anything by doing this, and will only waste a valuable word which might be used to express distinctions within experience.

Emp. But at any rate it seems to me that the occurrence of these events so different from ordinary feelings is not in any way

verifiable. When I try to detect them introspectively I cannot do so, and I do not see by what process we can *infer* a quite new kind of thing, as they would be, not analysable in terms of anything we immediately experience. And I do not see in the least why they should be called "acts", even supposing they occur.

Let us now consider the last remark of Empiricus, which brings us back to the most fundamental issue. We must admit in the first place that there is at any rate a certain inappropriateness about calling these phenomena "acts". I do not know how the term first came to be used for them, and I for my part wish it had not been chosen. It is unsuitable in two respects. Firstly, it suggests instantaneity, where what we should rather have in mind is a *process* of thought. Secondly, "active" suggests volition, and I think had better be reserved for cases where volition is involved. No doubt the experience of knowing or believing is commonly closely connected with volitions which play an essential part in bringing it about. In order to acquire knowledge or belief we have to direct our attention to something, and this direction of attention (usually, though not always) at some stage involves volition. But the actual knowing or believing is itself not willing. I can will to direct my attention in a certain way. I might even will to ignore certain inconvenient arguments, i.e. refuse to attend to them. But I cannot, strictly speaking, will to believe. Whatever the nature and place of free will in ethical action, free will does not come in here. And, even if I could will to believe or know, the believing or knowing itself would not be an act of will. It is therefore better to speak of cognitive states or processes than of cognitive acts.

Now that we can introspect cognitive processes, processes of thinking about something, surely cannot be disputed. An attempt is made to reduce these to imagery, including words, but surely the imagery does not merely occur? Surely we do something with it when we think, and the doing this is the mental process. The mere fact that imagery can only be used as meaning something, and that this something in very many cases cannot itself be translated in terms of imagery should be sufficient to show that we must admit over and above the images mental states and processes involving the apprehension of what is itself not an image but the meaning of an image. But it is a different thing to say we have a definite single mental act or state which occurs in all cases of knowing and could itself be called knowing, and similarly with believing. The answer to this question is, I think, that in some cases we have it and in others not. I sometimes have a definite experience at a given time of recognising

the truth of something or seeing something to be true. That might be called a mental act (if we ignore the suggestion of "activity"), as opposed to an ordinary mental process, for though not strictly instantaneous or timeless and therefore still a process, it sometimes seems to be completed in one specious present and is at any rate very much shorter than most mental processes of which we speak. But we need not suppose that such "mental acts" occur in all cases of knowledge or belief. Sometimes we just take something which we are said to know or believe for granted in our thoughts without ever (as far as we introspectively notice) expressly seeing it to be true. The critic of mental acts is right in holding that the terms "know" and "believe" are to be dispositionally interpreted, but I insist that they should be interpreted not, primarily at least, as dispositions to physical behaviour, but as dispositions to think. And, if we take the word "see" (in the sense in which we see the answer to a problem), this does not usually stand for a disposition, but rather for something such as those had in mind who asserted mental acts. I do not understand how anybody can deny our ability in some cases to discover introspectively that such events occur. And for reasons which I have given, I do not see how they can possibly be reduced to behaviour-tendencies or to imagery or both. Imagery is not enough; I must understand the imagery and see that it applies to something.

How is it, then, that there has been all this difficulty about "mental acts"? Is not the very fact that there has been such disagreement about them and that many experts have not (or think they have not) detected them introspectively an argument against them on the ground that, if they were there, they would have been noticed by everybody, as I made Empiricus urge in the second paragraph of this article? If we experience cognitive acts, why should we have doubts about their existence, when nobody doubts the existence of pains? Now it may, of course, just be an ultimate fact that the sort of phenomena of which we are talking are difficult to observe while other phenomena are not. If this is an inexplicable fact, it is only in the same position as any ultimate causal law, according to the empiricist at least, and therefore the empiricist in particular has no right to make it an objection against his opponent. Why should it not be an ultimate causal property of mental acts that they are harder to observe than, e.g., pains or visual sense-data, as it is an ultimate property of certain wave-lengths that they produce the sensation of yellow? The fact that others have failed to find them suggests that we should exercise extra care in making sure that we really

have detected them, but if we are sure either that we have done this or that they are necessary to explain our experience we need not be too upset by the fact that not everybody has succeeded in discovering them. The given argument against mental acts is therefore far from conclusive, but it should at any rate warn us to be cautious.

It is not, however, necessary to treat it merely as an ultimate inexplicable fact that some people find great difficulty in introspecting these mental events. The position is eased when we note the following points :

(1) There can be no doubt that everybody (except real behaviourists !) can introspect cognitions and beliefs as well as he can introspect pains, in the sense that he can become aware otherwise than by inference or the outer senses that he knows or believes something. The doubts regard the *analysis* of this fact of knowing or believing.

(2) Cognitive acts are too linked up with their objects for us to be aware of the acts by themselves, but we can be aware of them in conjunction with their objects. I cannot know what knowing or judging is like *per se*, because it does not exist *per se*, but I know what it is like to judge something or know something to be true. There would be similar difficulties about determining what the mental state of feeling pain is apart from the felt pain. It is not the former but the latter as a sensory datum, or rather the two in conjunction that we are thinking of when we speak of physical pain, while in the case of pain in the wider sense of "unpleasure" we are saying that our whole experience has the characteristic of painfulness. Nobody would think of looking for pain *in vacuo* apart from some experience which has other characteristics besides being merely painful, for pain in either sense is patently only a universal characteristic, therefore we are not tempted here to make the mistake which we make about acts of cognition when we look for them as if they were something separate and not merely distinguishable from the object cognised.

(3) No doubt, as has often been pointed out, the subjective side of cognition is apprehensible in a different way from either the imagery or the objective side. People who have denied the subjective side may have been looking for the wrong thing in the wrong way, expecting, like Hume, to find some sort of image or combination of images.

(4) Once we have taken the imagery away, we are left with something unanalysable that cannot be further described. But some concepts must be unanalysable, otherwise you have a vicious infinite regress, therefore we need not be put off by this.

(5) As we have seen, language raises many complications that obscure the issue. We must admit that the opponent of mental acts is partly right in that the term *act* is not appropriate as applied to cognitions, and that the words used of them are mostly dispositional, and are to some extent (though, I think, much less so than is very commonly supposed) liable to include in their meaning not merely a reference to the mental events themselves but to behaviour. In many cases of "knowing" and "believing" there is, I admit, probably no conscious act to be observed, only an unconscious taking for granted in our thought. These complications do not arise with "pain", which is not a dispositional term.

I have been discussing "cognitive acts", but it seems to me even more unreasonable to make the corresponding denial in the case of conations. I do not, however, say that an introspectible act of volition accompanies every overt action or every direction of mental attention, any more than that an "act of cognition" is present whenever we come to "know" or "believe" something. With actions where no real alternative presents itself volitions perhaps do not occur as distinguishable psychological phenomena. But where there are alternatives before our mind we can assuredly introspect choices either at the time of action (volitions), or prior to action, no immediate action on them being considered possible or desirable at the time (decisions). These can be suitably termed "acts" in a sense in which cognitions cannot, because they are directly brought into being in order to produce effects. But we should make a distinction between specific acts of choice and a continuous purposive process. The latter should not be reduced to a mere series of the former.

With cognition similarly we should distinguish a continuous process of thinking from particular "cognitive acts". The former should, I think, be regarded as basic rather than the latter, and the process of thinking out a problem should not be reduced to a mere series of such acts. The latter play rather the part of beacons and turning-points in the process than that of being the sole material which constitutes it. It is only at comparatively exceptional moments, as when we "see a new point", that we are aware of such an act, and we need not suppose that they are always occurring though we are not aware of them. In a sense I am indeed continuously aware that I am writing an article on mental acts and that I am in my study, but I need not therefore suppose that I am all the time performing an unintrospected act of awareness of my surroundings and my purpose like those "acts" which I detect introspectively when I ask myself a

definite question about them. To say that I am aware throughout of my environment and purpose (in the "background" of consciousness) is rather to talk about a relation which the process of thought continually bears than about a series of mental acts superadded to it.

But, after all, if we do discover "mental acts" by introspection, what is that but discovering some kind of feeling? What would it be like to be aware of a mental act that was not a feeling? Is not to know that I know or will just to know what it feels like to know or will? In a sense the people who take this line are right. To know what an experience is, is to know what the experience feels like, and it could not be knowing anything else in so far as it is a knowing of the experience itself and not merely of its objects, unless we include under "knowing what an experience is" knowing its causal properties. We are not, as might be held in the case of physical objects, knowing any property which could exist unfelt, for feeling is the determinable of which all particular experiences are determinate values. But we have not said very much when we have said this. It does not add much to the notion of an "experience" to say that it is a feeling in a sense of "feeling" so wide that it seems plausible to say that all experiences are just made up of feelings. It would indeed be saying a good deal if we could reduce all experiences to sensations of a recognisable kind which could be correlated with some definite sense-organs, but this cannot be done. Even if "feeling" does carry with it a vague reference to the body and even if we suppose that every event in our mental life may be correlated with some event in the brain, the correlation is at any rate not obvious to inspection as it is in the case of the pain caused by burning a finger or with sights and sounds. And if we say that everything in experience is a feeling or feelings, we must not confuse this wide sense of "feeling" with the more specific sense of "feeling" in which emotions and pains are feelings but judgements and volitions are not. Feeling in the former sense is synonymous with experience in general, in the latter sense it stands for very specific kinds of or phases in experience.

As a matter of fact, each of the three traditional basic terms of psychology has a wider and a narrower usage. If it can be contended that all conscious mental states are varieties of feeling, it can be likewise contended that they are varieties of cognition, as was done by Descartes. Any state of consciousness may from one point of view be regarded as a cognition, an awareness of something. Thought may obviously be brought under the

cognitive side, sensations have been commonly classed by psychologists as cognitions (of sense-data), pleasure and displeasure can be regarded as qualities of the cognitive states, and emotions towards an object as more complexly qualified awarenesses of that object or ways of thinking about that object. Of course, if we bring in their external manifestations, emotions are much more; but it might be contended that this is all they are on the inward side. Conation (desire and choice) presents most difficulty to such an account, but choices and decisions might be regarded as changes in our way of thinking of certain possible acts, and desires as tendencies to think in a certain emotionally toned way about certain objects. They also lead to action, but it is because we think first in a certain way that we act (except in cases of "automatic action", which is not in the full sense action. I do not say that this is the most natural, illuminating and satisfactory account of mental processes; but it is, I think, a possible one, and it brings out certain points of value.

Thirdly, we may make not feeling or cognition but conation fundamental in our account of mental life. We can indeed hardly make cognition and sensation just varieties of conation, but we may argue that conation is the fundamental aspect of the self on the ground that cognition is a function of attention and that pleasure and pain can themselves be analysed in terms of the conative attitudes of liking and aversion. To find an experience pleasant, it might be said, is to have a positive conative attitude to its quality as experience at the time it occurs, to find it unpleasant a negative. (Of course we might have a negative conative attitude towards the pleasant experience on other grounds, e.g. that we thought it would have bad effects or was based on something of which we morally disapproved.)

The truth seems to be that we may use each of these three terms, conation, cognition, and feeling, either in a wider or in a narrower sense. If we use it in the wider sense, each stands for an aspect of all events in our mind and not for a species of mental events to which some belong and not others. Thus we might think of feeling as constituting the stuff which makes up all our mental life. We might perhaps think of cognition as constituting a certain relation of our experience to the real (including sense-data, which we must remember are not *parts* of mind, and in the case of introspection an aspect of the mind itself other than the aspect we are describing as a cognition of it). And we might think of conation as constituting the connecting-link which organises all our experience (cf. Leibniz). Attention is guided by purpose, and attention organises our mental content and decides

what we shall cognise ; our temporary and our permanent dispositional purposes determine what we shall think and feel.

But the narrower sense is at least equally legitimate. If pains, emotions, and the seeing of answers to problems are all felt and so in a sense feelings, the third is so different from the other two classes that it deserves a different name. To call it a feeling may indeed have disastrous philosophical results, as it did with Hume, for whom it connoted that all beliefs, being mere sensations or feelings, were strictly speaking irrational (though scientific beliefs were still more rational in some relative sense than superstitions). Even if the three aspects be present in all human experiences, at any rate one is liable to be more prominent than the other two. If we experience a twinge of toothache we know that we have it, but that aspect is subordinate to the feeling. When we discover a particular emotionally indifferent fact we feel something, but feeling is subordinate to the knowing, and when we attend in order to find out the truth about something conation is present, but it is a mere subordinate means of cognition. When we choose or desire something we are aware of what we choose or desire, but the fundamental, the interesting fact is our conative attitude towards the object, not the mere fact that we are aware of it and think of it. So cognitions, conations and feelings may be also used as class terms for species of mental events as well as for different aspects present in all mental events. And the term used in the narrower sense of a mental event will be selected according to which of the three aspects predominates in that event. Its use will not deny the presence of the other two aspects in the event, but it will imply that in this event they are unimportant or at least subordinate.

Cognitive processes are thus felt experiences, but they deserve treatment as a special class among felt experiences. If you call them feelings, you have to admit that they have the property of yielding truth and falsehood in a unique way which is not common to all experiences. And, if you admit this, it does not perhaps matter very much whether you do or do not call them feelings, provided you are careful to explain how different they are from other "feelings". A "cognitive act" we may then regard not indeed as something which exists *per se* in its own right but as a real element abstracted from a cognitive process, and perhaps the same applies to conative acts *mutatis mutandis*.

VIII.—DISCUSSIONS.

DOES ARISTOTLE HAVE A DOCTRINE OF SECONDARY SUBSTANCES ?

THE question that serves as a title for this note is of course to be answered in the affirmative. That the passages in the *Categories* treating of secondary substances constitute Aristotelian doctrine is indisputable, except possibly on philological grounds.¹ It is noteworthy that *δεύτεραι οὐσίαι* are never mentioned, at least by name, in the Aristotelian corpus outside of the treatise on categories;² none the less the passages in the *Categories* concerning secondary substances justify, by themselves, the use of such a word as 'doctrine', and, when coupled with Aristotle's denial, in the *Metaphysics*, of the attribution of substance to universals, demand explanation.

Hypotheses that can explain the apparent irreconcilability of the *Categories* and the *Metaphysics* on this point might be of several types. The treatises may be actually contradictory, that is, the *Categories* may be taken as asserting that some universals, namely species and genera, are substantial, whereas the *Metaphysics* may be understood as asserting that no universals are substantial. Similarly it may be urged that the irreconcilable character of the two treatises, while not to be gainsaid, reflects earlier and later phases of Aristotle's thought on the issue. This second type of explanation, though it may be correct, is only historical. It does not address itself to the problem of the interpretation of Aristotle the philosopher, but rather to a conjectural calendar of the thoughts of a man from Stagira. Such an "explanation", as is well known, is very much the vogue.³ A third kind of explanation, and, it may be urged, the most hopeful, is one that seeks to show that the discussions in question are not contradictory, if justifiable allowances as to the purpose and scope of the *Categories* are made. The allowances required are simply those that are implicit in a sufficient regard for the contexts in which Aristotle's apparently conflicting statements occur.

A few remarks by diverse interpreters of Aristotle may be helpful in setting the stage for the recommendations to follow. H. P. Cooke, prefatory to his translation of the *Categories*,⁴ asserts: "What is the subject of the *Categories*? In ordinary usage *κατηγορία*,

¹ Cf. W. D. Ross' estimate of the controversy as to the authenticity of the *Categories* in his *Aristotle*, 2nd ed., London, 1930, pp. 9-10.

² Cf. H. Bonitz, *Index Aristotelicus*, p. 544, col. 2.

³ Cf. e.g. any of the writings of Werner Jaeger on Aristotle.

⁴ Aristotle, *The Organon*, vol. 1, Loeb Library, London, 1938, p. 2.

rendered in English as 'category', meant nothing more than 'a predicate'. This meaning it seems highly probable that it retains in the text. The ten categories, then, are ten predicates."

In order to rescue the logical aspect of the *Categories* from Cooke's grammatical emphasis, and relate the *Categories* to the ensemble of treatises that culminates in the two *Analytics*, Hamelin's words, in part a citation of Ammonius, are apropos :

"Nous voulons connaître la démonstration. Mais la démonstration est une sorte de syllogisme ; nous devons donc chercher, avant de nous attacher à la démonstration, ce que c'est que le syllogisme pur et simple. Le syllogisme pur et simple est, comme l'indique son nom, quelque chose de composé. . . Il faut donc commencer par apprendre de quels éléments il se compose : ces éléments sont les propositions. Celles-ci se composent de sujets et d'attributs. Ainsi nous sommes amenés à considérer comme l'objet initial de la logique l'étude des notions séparées dont la proposition est l'assemblage. En un mot la logique doit débuter par l'étude des catégories. C'est cette étude qui va maintenant nous occuper."¹

Boethius' summary observation, that the *Categories* is concerned with forms of discourse rather than with kinds of things *in rerum natura*,² is also germane. The point I hope I have established by the foregoing quotations is that it is granted on all sides that the *Categories* is a discussion of *terms*.

Yet though the *Categories* is primarily intended as a theory of predication, it may be urged that whenever remarks of a definite ontological bearing occur, it would be more than foolish to ignore them. Moreover, the discussions of secondary substance appear to have just such a bearing. They contain two related difficulties for any view that holds that the *Categories* is a logical work, devoid of ontological doctrine. First, why does Aristotle call 'species' and 'genera' *οὐοῖαι*? Secondly, does not the statement that species are more substantial than genera imply a definite ontological hierarchy?

An answer to the first question is provided by a consideration of what Aristotle takes to be the discursive function of the terms 'species' and 'genera'. The terms 'species' and 'genera' are labelled *οὐοῖαι* because they do not signify merely qualitative determinations, but rather the qualitative determinations that define primary substances. The predication of genus and differentia is a statement of "what sort a substance is".³ Thus the rôle of such terms in Aristotle's logic entitles them, as opposed to other kinds of predicates, to the name 'secondary substances'.

The problem concerning an ontological hierarchy is resolved along the same lines : "The statement that of secondary substances the specific term is more truly substance than the genus, means only

¹ O. Hamelin, *Le Système D'Aristote*, Paris, 1920, pp. 96-7.

² In *Cat. Arist. I* (Migne PL64, cl. 169A-B); cited by E. A. Moody, *The Logic of William of Ockham*, New York, p. 127.

³ Cf. *Cat. 3b20*.

that the species *signifies* individual substances more determinately than the genus.”¹

In sum, Aristotle has a definite doctrine of secondary substances. The only explicit source for the doctrine, the *Categories*, is concerned with predication, in particular the distinctive features of incomplex terms. These terms are set forth as predicate of, or not predicate of primary substances, the ultimate subjects in the Aristotelian logic. What is more, in the *Categories*, ‘primary substance’, like ‘secondary substance’, refers to a term, not to an extralogical entity or entities.

‘Secondary substance’, as the following section may show, has no ontological analogue in the *Metaphysics*.

2

The foregoing, limited as it is to Aristotle’s theory of predication, can hardly pose as conclusive concerning any questions about the Aristotelian ontology. The court of last resort for decisions as to Aristotle’s philosophy of Being is necessarily the *Metaphysics*. To rephrase the question with which this note began, does Aristotle have a doctrine of secondary substances in the *Metaphysics*?

The fact that δεύτερα οὐσίαι are not mentioned by name in the *Metaphysics* by no means disposes of the problem. It merely pushes the inquiry to the level of seeking traits of substance such that species and genera are or are not admissible as substances.

It is commonly acknowledged that Aristotle’s view of substance stresses the individual, the *tode ti*. This emphasis, it is no doubt correctly contended, distinguishes him from Plato. Yet since Aristotle’s account of substance in the *Metaphysics* is highly perplexing, there is a considerable divergence of opinion as to the sense and extent of his insistence upon the individual as substance. For example, does he thereby withdraw the appellation ‘substance’ from the Platonic forms? If in some passages he appears relentlessly opposed to any doctrine of substantial ideas, is he in the last analysis thus opposed?

Zeller, approvingly quoted in Russell’s *History of Western Philosophy*,² claims that, “The final explanation of Aristotle’s want of clearness on this subject is, however, to be found in the fact that he had only half emancipated himself, as we shall see, from Plato’s tendency to hypostatise ideas. The ‘Forms’ had for him, as the ‘Ideas’ had for Plato, a metaphysical existence of their own, as conditioning all things.” In any comparison of Aristotle and Plato on the subject of “hypostatised” ideas, it should be noted that Aristotle, unlike Plato, never suggests the hypostatisation of moral qualities, numbers or quantitative relations, or what were

¹ Moody, *op. cit.*, p. 138; my italics.

² p. 166; from Zeller’s *Aristotle*, vol. 1, p. 204.

later called 'transcendentals'. In the *Categories* Aristotle gave the name 'secondary substance' to species and genera alone.

The question as to substantial species and genera remains. Some brief reminders from the *Metaphysics* constitute a sufficient answer. To begin with, Aristotle therein flatly denies the attribution of substance to genera: *οὐτε τὸ καθόλου οὐσία οὔτε τὸ γένος*¹ ("... neither the universal nor the genus is substance"). Since species are universals, the statement applies to them as well as to genera. For those who may insist, somewhat doggedly, that 'universal' is employed in the quoted phrase as synonymous with 'genus', additional evidence can be brought forward. But already it should be noted that Aristotle's "half-emancipation" from Platonic realism explicitly involves the rejection of any substantial ideas or forms, excepting possibly *infima species*.

Concerning *infima species*, Mure, at the end of his discussion of Aristotle's treatment of substance,² asserts: "Hence in Met. VII, where Aristotle debates with masterly penetration how to meet the claim of substance to be at once universal, intelligible, and definable, and at the same time unique and individual, the singular is at length superseded on the throne of substance by the *infima species*." Waiving doubts as to whether Aristotle would attempt such a brazenly contradictory feat, it should be pointed out that Aristotle definitely rejects the pretensions of *infima species* to the "throne" of substance. At 1029 *a* 28 he says: *καὶ γὰρ τὸ χωριστὸν καὶ τὸ τόδε τι ὑπάρχειν δοκεῖ μάλιστα τῇ οὐσίᾳ*³ ("... it is accepted that separability and individuality belong especially to substance.") But forms or species are denied both separability and individuality:

*Πότερον οὖν ἔστι τις σφαῖρα παρὰ τάσδε ἡ οἰκία παρὰ τὰς πλίνθους ;
ἢ οὐδὲ ἀν ποτε ἐγίνετο, εἰ οὐτως
ἢν, τόδε τι, ἀλλὰ τὸ τοιόνδε
σημαίνει, τόδε δὲ καὶ ὡρισμένον
οὐν ἔστιν, ἀλλὰ ποτεῖ καὶ
γεννᾶ ἐκ τοῦδε τοιόνδε · καὶ
σταν γεννηθῆ, ἔστι τόδε τοιόνδε.⁴*

("Is there then some sphere besides the particular spheres, or some house besides the bricks? Surely no individual thing would ever have been generated if form had existed thus independently. Form means 'of such a kind', it is not a definite individual, but we produce or generate from the individual something 'of such a kind', and when it is generated it is an individual 'of such a kind' .")

According to Aristotle, then, whatever is neither separable nor

¹ 1042 *a* 22; the translations are by the Loeb Library translators.

² Aristotle, New York, 1932, p. 187.

³ Cf. also 1017 *b* 23-27.

⁴ 1033 *b* 20-24.

individual is not a substance ; forms or species are neither separable nor individual ; therefore forms or species are not substances.

In conclusion, then, the assertions of both the *Categories* and the *Metaphysics* concerning universals are compatible on the foregoing interpretation. In the *Metaphysics* Aristotle denies that any universals are substances ; in the *Categories* the characterizations 'secondary substance' and 'primary substance' refer to terms, and not to substance in an ontological sense. The significance for the history of philosophy of this interpretation is that one, perhaps the chief prop for the classification of Aristotle's ontology as moderately "realist", or as "half-emancipated" from Platonic realism, proves baseless.

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DAVID SACHS.

INDUCTION AND THE FUTURE

"WILL the Future be Like the Past?" inquires Professor Frederick Will, in a recent engaging essay.¹ And he argues that it will, by imputing to Hume, Russell, and other 'sceptics' a fallacy so transparent as to constitute a quite astonishing negligence on their part, if he is right, and on his part, if he is wrong.

The sceptical critics of induction, Mr. Will correctly reminds us, have contended that the observed uniformity of nature, and the consequent success of induction, at past times cannot make credible a present induction, extending to a future now impending, without assuming the main point at issue, that inferences from past and present to the future are in general valid. Mr. Will retorts that the fact that in 1936 I made a prediction about 1937 which was borne out by the event, the fact that in 1937 I made predictions about 1938 which were borne out, and so forth, do confirm the principle that inferences from the data afforded by any year n to conclusions about a succeeding year, $n + 1$, are in general reliable, and that, still more generally, the circumstance that the objects described by the conclusion of an inductive inference are located at times later than the times of the data described by its premises, does not discredit the inference. Mr. Will thinks that the sceptics have missed or mistaken this principle because of a slipperiness of the word "future". He thinks that they have believed that no past experience of successful prediction can be relevant to further predictions now for one or more of the following reasons. First, no prediction realised in 1938, for example, can illustrate a general cogency of inferences from past to future because, by the time it was realised, it was no longer about a future but about a present. Secondly, no predictions realised up to the moment when the sceptics wrote their critiques, or the moment when Mr. Will wrote his, or the moment when this passage is written or read, can illustrate inference from past to future, because the vocabulary of this moment admits only one 'future', namely, what succeeds *this* in the time series, and no instance of such futurity has yet occurred. But finally, since at every moment hereafter we can mean by "the future" only events which have not yet occurred, statements concerning 'the future' can never have any application. 'The future' in this sense never arrives; it can never be put in evidence, but by the same token can never be an object of any sane man's anxiety or anticipation.

Now, Mr. Will has called attention to an error which some persons might make, but I am confident that Hume did not make it. I

¹ Frederick L. Will, "Will the Future be Like the Past?", *MIND*, vol. lvi, no. 224 (October, 1947), pp. 332-347.

am especially confident that Russell, with his elaborate doctrine of 'egocentric particulars' (a category which includes 'the future' as well as 'I', 'now', 'this', 'here', etc.), could not have made it. The sceptics' argument is fundamentally different from what Mr. Will supposes. They are on occasion perfectly willing to mean by "future" the general status of being later than some other time, any other time; ready to admit that 1938 is (in the timeless or logical sense of "is") future to 1937; ready to write in terms of the general question whether inferences are valid from year n to year $n + 1$. They would grant, therefore, that over and over again, a present (if one wishes to use such phrases)¹ has given place to a *bona fide* future, and would grant, therefore, with no cavil about the future's 'changing', that inductive inferences from presents to futures have been often substantiated by the event. They are prepared to agree, when 1949 comes round, that *it* is the 'future' they were talking about in 1948. And certainly they would concede that if we were assured of the fundamental principle that what is true of a sample or subclass of any population, whether the population consists of inferences about futures, of rats, of cats, or of what not, can be reasonably asserted of the rest of the population, then our experience with one batch of futures would provide a valid ground for inferences concerning another, unexplored batch. But they object that the population of inferences about futures (*i.e.*, inferences from times n to times $n + 1$) is in exactly the same boat as other inductive populations in this respect. We can know nothing about it except by assuming the general inductive principle, and hence nothing we know about it can validate the inductive principle.

The sceptic's objection, in brief, is not, as Mr. Will contends, that the situation composed of to-day's present and its future is not similar to past present-future situations, nor that to-day's future will never come. The objection is that the two situations, though equally real and genuinely similar segments of the spatio-temporal continuum, are numerically distinct from one another. For the tragic puzzle of induction has been just the question whether and how any one set of facts furnishes a valid basis for an inference concerning another set of facts. The principle that the future resembles the past was supposed to guarantee such a basis. The sceptical objection is that this principle is confirmable only by assuming that the basis already exists; and not all Mr. Will's semantics does anything to mitigate this vicious circularity.

¹In mentioning 'present presents', 'past futures', and so forth, I follow the somewhat happy-go-lucky example of Mr. Will and Bertrand Russell. The phrases do not parse, but are readily translatable. More dangerous are expressions like "the present is a constantly changing one" and "the movement of time" (p. 345). *Change* in general consists in the occupancy of a given region at different times by different characters. To give the name "change" to the fact that different times are different times is an invitation to trouble.

Mr. Will can so widely mistake the intention of the inductive sceptics because he belongs to that interesting and influential party, so often represented in the pages of MIND and not unknown in the United States, who believe that philosophical problems are to be cured rather than solved, and that they are to be cured by restoring the patient to the Elysian complacency of common sense. Common sense takes induction for granted, and Mr. Will accepts without argument or apology the one thesis which our sceptics are interested in debating, namely, that inductive confirmation is valid. He must then treat the *philosophy* of the subject as a morbid departure from the norm, and attack it by psychoanalysis and word drill.¹ This method was worth trying, but I think that now its day in court has been long enough. Its merits have not been demonstrated, and its dangers are conspicuous. Psychologically, it has appeared, Mr. Will was ingeniously mistaken about his patients' motives. Logically, I suggest, he is none too exact about the consequents of his own main assumption, the inductive principle.

Unless we assume the inductive principle, we have observed, the success of past predictions is wholly insufficient to give any credibility to a present prediction. If we do assume the inductive principle, let us observe finally, the success of past predictions is quite unnecessary to give credibility to a present prediction. The most it can do is to add slightly to the credit of a prediction already well confirmed. We might have found, empirically, that what happens in year n is not generally a good clue to what happens in year $n + 1$, just as we have found that the political opinions of automobile-owners are not very symptomatic of the political opinions of non-owners. In fact, however, we have found that year n has pretty consistently foreshadowed year $n + 1$, so that, granted always the general principle of induction, my inductions from the weather of 1947 and preceding years, to the weather of 1948 and succeeding years, fall heir not only to whatever rational credibility they get directly from the *weathers* so far observed, but also to an increment of credibility (very difficult to assess) which they get from past *predictions* and their success.

Even when the cardinal inductive principle is assumed and exploited in this manner, however, Mr. Will is wrong in supposing that there is no significant difference between a present present and a past present. Even if the present present were exactly like some past present (it could not be exactly like them all), the fact that it remains numerically distinct would entail that there is no impossibility, but only an improbability of some high degree, that its consequents be different. But the present present is not only

¹ Without assurances to the contrary, we may suppose that Mr. Will's present position remains near to that of "Is There a Problem of Induction?" in *The Journal of Philosophy*, vol. xxxix, no. 19 (September 10, 1942), pp. 505-513, where he argues, for example, from 'the ordinary sense of "know"' that we must know about the future.

numerically distinct from past presents ; it is qualitatively different, in numberless respects, from past presents, and the force of the evidence which the latter gives us is correspondingly diminished.

Like Charles Peirce, I think that there is not much good to be had from arguing whether nature is uniform and whether the future is similar to the past. The question is vague. The answers returned have usually been vague too, and when not vague, they have been either incapable of justifying induction, or incapable of being justified on their own account, or both. Neither on Mr. Will's view, according to which the inference from sample to population does not need to be validated, nor on mine, according to which it needs to be and has been validated, nor on the sceptics', according to which it needs to be but has not been validated, does the distinction between past and future, past pasts and past futures, and the like, or the fact that samples frequently preserve much the same statistical composition as they grow bigger with the passage of time, make enough difference to justify very fervent contention.

DONALD WILLIAMS.

NOWELL-SMITH ON RETRIBUTION AND RESPONSIBILITY.

IN an article on "Free-Will and Moral Responsibility" in *MIND* for January, 1948, Mr. P. Nowell-Smith puts forward the theory that a "morally responsible" action is one which the agent can be induced to do or deterred from doing by reward and punishment, praise and blame. He finds that his theory fails to account for the retributive element in punishment, and particularly for the custom found in some societies of punishing a wrong-doer without regard to "what we call the morality of the action, which includes the condition that it be voluntary", so that idiots, animals and even inanimate objects may receive punishment for actions held to be bad. As an explanation of this custom along utilitarian lines, Nowell-Smith suggests that it is the result of holding a particular theological view, *viz.* that God, or the gods, "require certain sorts of conduct on the part of human beings, and that they will visit breaches of the rules with their wrath in the form of plagues, famines and other undesirable events". Thus, the punishment of unintentional wrong-doing has for its purpose to avert the wrath of Heaven. Nowell-Smith notes that the penal system and the theological view in question are generally found together, and that when the theological view is abandoned, the penal system is abandoned too.

I do not think this explanation will do. For if the "eye-for-an-eye" theory of punishment is to be the effect of the belief in a jealous God, then the belief in a jealous God must have arisen independently of it; *i.e.* people must have come to believe that God held a crude retributive view of punishment, although they did not hold such a view themselves. What reason could there be for believing that God held such a view? And what reason could there be for ceasing to believe this? I cannot suggest any likely source for such an interpretation of the divine nature except an analogy with human nature; and unless I find very convincing reasons to the contrary, I must assume that people assigned a crude retributive view of punishment to their gods because they took such a view themselves, and ceased to assign it to their gods because they had ceased to hold it themselves. Theological beliefs of this sort are not abandoned because of empirical evidence (there are always plenty of misfortunes which can be explained as divine retribution); they are abandoned because they have become morally intolerable. Thus Nowell-Smith's account of the matter seems to me to put the cart before the horse. If he is right, a doctrine of a jealous God precedes a crude retributive system of punishment, and the abandonment of the doctrine precedes the abandonment of the penal system. If I am right, it is the other way round. Now the origins of barbaric theology are not historically traceable; but there is some historical evidence of the decline of this theology. The evidence suggests to

me that men have abandoned crude retributive methods of punishment before they have abandoned the notion that God employs such methods—religion being here, as in so many other respects, conservative. In the same fashion the doctrine of Hell was still held in the nineteenth century in spite of its inconsistency with current ideas of justice, but has been widely abandoned even by Christians in the twentieth.

This point is not to be taken as an argument against Nowell-Smith's general thesis, with which I am on the whole in agreement. For the explanation of the retributive attitude to punishment I would call attention to two factors. One is the desire for retaliation upon people who do actions which have evil results, a desire which at a relatively low level of reflection takes little or no account of the "moral responsibility" of the agent; resentment may perfectly well arise at the harmful action of an idiot or an animal as well as of a rational human being (the "punishment" of inanimate objects is more likely to be due to an attempt at theoretical consistency). This resentment gives rise to a drive, tension or uneasiness which is not satisfied until the offender has made some kind of expiation (it is sometimes satisfied by apology or repentance in lieu of punishment, but it remains to disturb us if the offender goes scot-free). I am sure that this spontaneous resentment, and not any calculation of results, is the main ingredient in the retributive attitude to punishment. There can be few people in this country who did not feel a satisfaction, as of an outstanding account closed, on hearing of the suicide of Hitler among the ruins of his Reich. In proportion as we get rid of this feeling of resentment, and replace it either by a sentiment of sympathy for the wrong-doer, or by a wish to discover the best method of preventing crime, to the same extent the retributive theory of punishment ceases to appeal to our "intuitions". The second point to be borne in mind is this. Our first notions about reward and punishment and moral responsibility are acquired when we are small children being disciplined by our parents. In that disciplining process the main aim of the parent is to secure the reliable performance or omission of certain types of action; and children are, of necessity, constantly being punished for actions done without evil intent, and often enough for pure accidents. So one would expect *a priori* that children would get the idea that punishability has everything to do with the results of action and nothing to do with the state of mind of the agent. Empirical confirmation that children do think in this fashion is provided by the investigations of Piaget. Later, as he shows, they grow out of this way of thinking; but they do not grow out of it completely. It is not unreasonable to suppose that the less sophisticated societies which believed in crude retribution had not grown out of it as far as we have; and, in particular, that their thought about the gods, which one might expect to contain many survivals of attitudes originally taken up towards parents, contained this attitude to punishment.

C. H. WHITELEY.

FACTS AND ENTAILMENT.

IN the last issue of MIND Mr. Arthur Prior defended a view of entailment put forward in a previous issue by Mr. Malcolm, and subsequently criticised by me. Mr. Prior describes the view in these words: "when one proposition entails another, this does not reflect some mysterious relation between distinct facts, but means rather that the fact expressed by the consequent is identical with that expressed by the antecedent, or at least is 'not a new fact in addition to' the first". This manner of expressing the view makes it necessary for me to explain the general purport of my past and present criticisms, which by no means support the alternative mentioned, *viz.* that entailment reflects some mysterious relation between distinct facts. So far as the association of entailment with a certain sort of relation is concerned I am in agreement with Mr. Malcolm and Mr. Prior, *i.e.* I agree that the relation is one of identity (if I may take the liberty of calling identity a relation) or of 'being contained in'. The disagreement arises over 'fact', *viz.* over the question whether we are at all justified in supposing that the terms of the relation expressed by entailment are distinctively *factual* in character, whether one fact or different facts.

Mr. Prior's amended definition of entailment is: "when one proposition entails another, then whatever fact is expressed by the consequent is either identical with or contained in a fact expressed by the antecedent, and whatever fact is denied by the antecedent is either identical with or contained in a fact denied by the consequent". The use of the word "whatever" in the *definiens* introduces a hypothetical sense which I think was not intended, for it would defeat Mr. Prior's purpose to provide for the possibility that there may be no fact expressed by the antecedent or consequent of an entailment, and no fact denied by them either. The second part of the *definiens* was introduced to ensure a reference to some fact (*viz.* by denying it) in case neither the antecedent nor the consequent expressed any fact. For example, if John does not own a horse we have the entailment "'John owns a mare' entails 'John owns a horse'", in which neither the consequent nor the antecedent expresses a fact, but in which (so I presume the argument would run) they do each deny a fact. This shows that the *definiens* is not really a conjunction but a disjunction. In this disjunction we have to distinguish the alternative in which the consequent is true but the antecedent false, for the factual situation in this case differs significantly from the factual situations in those two cases in which the antecedent and the consequent are both true or are both false. The entailment of 'John owns a horse' (*q*) by 'John owns a mare' (*p*) is therefore to mean either:

- (1) ' q ' expresses a fact which is contained in a fact expressed by ' p ' (here both ' p ' and ' q ' are true), or
- (2) ' p ' denies a fact which is contained in a fact denied by ' q ' (here both ' p ' and ' q ' are false), or
- (3) ' q ' expresses a fact which is identical with a fact expressed by a proposition contained as a component in ' p ' (here ' q ' is true but ' p ' is false. The false proposition 'John owns a mare' is 'John owns a horse and his horse is female', which contains the true proposition 'John owns a horse' as a component).

These alternatives correspond exactly to the three alternatives in the truth-table definition of material implication, but they are different from the truth-table definition because they involve the relations of identity and 'being contained in'. The three alternatives are mutually exclusive, and each is distinguished by reference to a peculiar factual situation. The meaning of " p entails q " would therefore have to be different, according to which of the three alternatives was realised, if the reference to facts is to remain an essential part of the definition. In consequence, we could not ensure that 'John owns a mare' entails 'John owns a horse' without considering separately the three possible meanings, for the entailment as it stands would be ambiguous. As against this, however, it is certain that we do not have to consider three separate meanings before we are able to decide whether the entailment holds, for the question is settled immediately by one consideration alone, *viz.* that every mare must be a horse.

The fact that the relation of 'being contained in' occurs in all the three alternatives, but that in (3) it relates propositions instead of facts, strongly suggests that the one meaning which we are looking for is logically connected with the relation of 'being contained in', but is not necessarily associated with 'facts'.

Next let us examine more closely the second alternative meaning in this example, *viz.* that in which John does not own a horse. Since the antecedent is affirmative, we are bound to produce a *negative fact* in order to uphold Mr. Prior's position. Whatever our views about negative facts may be, in these circumstances their introduction clearly has an *ad hoc* character. For if we are to express, in terms of the word 'fact', the statement that a certain proposition (e.g. 'John owns a mare') is not true, all we have to say *prima facie* is that it does not express any fact, not that there is a certain fact which it denies. There seems to be no obvious reason why there should be a fact denied by it. If there were such a fact it might provide us with a ready method of *knowing* that the proposition is not true; but there might be other ways of knowing its untruth, and in any case this is not what its untruth means. Its untruth means merely that it is not true, *i.e.* that there is no fact of such a kind as to make it true, and this is very different from saying that there is a fact of such a kind as to make it *not* true.

This objection does not rest upon a peculiar definition of 'not true'. If we are going to produce various orders of fact *ad lib* according as they are required, it is clear that the empirical basis intended by the truth-functional account of entailment has vanished, even though the story in terms of 'fact' be prolonged to meet all emergencies.

Mr. Prior's position, we have found, depends at least upon his ability to substantiate the claim that in all cases of entailment either there is a fact expressed by the consequent or there is a fact denied by the antecedent. Consider "'John will buy a mare to-morrow' entails 'John will buy a horse to-morrow'". In this case, if there is a fact expressed by the consequent, it must be settled now that John will buy a horse to-morrow, while if there is a fact denied by the antecedent, it must be settled now that John will not buy a mare to-morrow. In other words, Mr. Prior's position cannot be upheld except by insisting upon a rigid determinism, and it seems odd to defend a logical account of the meaning of entailment by the metaphysical doctrine of determinism. It might be said that although there is no fact of either kind now, one will be produced in the course of to-morrow's happenings. The reply is that the entailment holds as indubitably to-day as it will the day after to-morrow.

I suspect that Mr. Malcolm and Mr. Prior defend a definition of entailment in terms of fact because on the one hand they believe, rightly I think, that the relations of identity and 'being contained in' are logically connected with the relation which they call entailment, while on the other hand they assume, mistakenly, I think, that identity and 'being contained in' are peculiarly associated with facts. Either of these two relations *may* relate facts, *e.g.* the fact that John owns a horse and the fact that he owns a mare, but they can also relate propositions, *e.g.* the proposition 'John owns a horse' and the proposition 'John owns a horse and John's horse is female'; they may also relate classes, *e.g.* the class of mares and the class of horses; and they may also relate concepts, *e.g.* the concept 'horse' and the concept 'female horse', or the concept 'dog' and the concept 'spaniel'. On the basis of the relation of identity or 'being contained in' we may still derive an entailment, even though the terms of the relation are not facts, *e.g.* from "'p' is one of the propositions 'q' or 'r'", *i.e.* from "'p' is contained in 'q . r'" we derive "'q . r' entails 'p'", and from "'dog' is contained in the meaning of 'spaniel'" we derive "'Fido is a spaniel' entails 'Fido is a dog'", although 'dog' and 'spaniel' are not facts but concepts.

It seems clear from this that Mr. Prior regards as necessarily relating facts, certain relations which admittedly relate facts more conceivably than entailment relates facts, but which may also relate other sorts of entities. All we have in effect is the logical derivation of an entailment from a proposition asserting an identity

or asserting that one term is contained in another. The principle of substitution expresses, in a general form, a system of logical derivations of this kind. For our purposes we may write the principle as:

$x = y$ entails “ $(\phi)(\phi x$ entails $\phi y)$ ”.

It is only the second of these two entailments that has been discussed by Mr. Malcolm and Mr. Prior, for we have found that they are only concerned with the kind of entailment which can be logically derived from a proposition expressing identity or the relation of ‘being contained in’. As far as I can see they have not noticed that the logical derivation is itself an entailment though of quite another kind. To avoid confusion we must now distinguish between these two kinds of entailment. The kind expressed by the first occurrence of the word ‘entails’ in the above expression for the substitution principle we shall call ‘entailment (1)’, and the kind expressed by the second occurrence ‘entailment (2)’. Professor Moore’s conclusion in regard to entailment (2) in his essay on External Relations in *Philosophical Studies* is that it is strictly not entailment at all, and with this I am in agreement. But he regards “‘John owns a mare’ entails ‘John owns a horse’” as expressing a genuine entailment, *i.e.* he apparently does not observe that this entailment is derived in the following way:

“‘horse’ is contained in the meaning of ‘mare’”
 entails “‘John owns a horse’ is contained in the meaning of
 ‘John owns a mare’”,
 which entails “‘John owns a mare’ entails ‘John owns a
 horse’”.

The form of this derivation is closely analogous to the form of the substitution principle, the only difference being that we have the relation of ‘being contained in’ in place of identity. The entailment of ‘John owns a horse’ by ‘John owns a mare’ therefore plainly falls under the heading of entailment (2). For our purposes we need not, fortunately, probe the question whether entailment (2) is strictly entailment. What is more to the point is that, even though not an entailment, it is not a material implication either, for it is a function not of the truth or falsehood of its component propositions (*e.g.* ‘ ϕx ’ and ‘ ϕy ’, assuming ϕ , x , and y to be given particular values; or ‘John owns a mare’ and ‘John owns a horse’) but of the truth or falsehood of an extraneous proposition (*e.g.* ‘ $x = y$ ’, or “‘horse’ is contained in the meaning of ‘mare’”). It is equally clear that entailment (1) is not a material implication, for its truth is a function not of the truth or falsehood of its component propositions, *e.g.* ‘ $x = y$ ’ and “ $(\phi)(\phi x$ entails $\phi y)$ ”, but of the formal structures of these component propositions.

It is an easy matter to think that an entailment derives from the facts themselves, when in truth it derives only from the form

of the facts, and still derives from the form when the matter is not factual. To take an example involving a somewhat different set of logical relationships, it is easy to think that, when John in fact does not own a horse, the entailment of 'John owns a horse' by 'John owns a mare' derives from the fact of his not owning a horse and the contained fact of his not owning a mare. But the logic of the derivation is precisely the same if we begin from the premiss that the *proposition* 'John does not own a mare' is contained in the *proposition* 'John does not own a mare, and does not own a stallion, etc.', i.e. in the proposition 'John does not own a horse'. Thus:

" 'John does not own a mare' is contained in 'John does not own a horse'"

entails " 'John does not own a horse' entails 'John does not own a mare'"

which entails, by transposition,

" 'John owns a mare' entails 'John owns a horse'".

The steps involved here are of a purely formal character, *viz.*

" ' $\sim p$ ' is contained in ' $\sim q$ '"

entails " ' $\sim q$ ' entails ' $\sim p$ ' "

entails " ' p ' entails ' q ' "

and the introduction of a factual content at any point is arbitrary. Consequently such a factual content cannot be regarded as contributing either to the derivation of an entailment or to its definition. The factors relevant to the derivation of entailment, and conceivably to its definition, are the relations of identity and 'being contained in' (regarded as purely formal) and, for a sort of entailment not discussed by Mr. Malcolm or Mr. Prior, the formal structures of the constituent propositions of the entailment.

I have discussed only the first half of Mr. Prior's article. But the second half depends upon the definition enunciated in the first, and if the principle underlying the definition is mistaken, the position as a whole can evidently not be repaired by making further applications of the same principle.

E. TOMS.

IX.—CRITICAL NOTICES.

La Vita, le opere, i tempi di Edoardo Herbert di Chirbury. By MARIO M. ROSSI. Three volumes. Florence, 1947. Price, lire 5000.

SIR WILLIAM HAMILTON, in the course of his discussions on the works of Reid, called attention to the philosophy of Lord Herbert of Cherbury, saluting him as a prophet of the doctrine of Common Sense. A monograph on his conceptions by Charles de Rémusat appeared in 1874 and a further exposition of his chief philosophical work, *De Veritate*, was published by C. Gütter in 1897. Professor W. R. Sorley, in the meantime, had contributed an article on his ideas to MIND in 1894. Recently, in 1937, the present reviewer rashly attempted to translate *De Veritate* and to offer some appraisement of its argument. And Herbert's theological views have been lately discussed by Mr. H. R. Hutcheson of Yale in his edition of *Religio Laici*.

These and other inquiries that could be mentioned have now been eclipsed by the appearance of Signor Rossi's immense exploration of Lord Herbert's life and writings. In three large volumes, each of which falls little short of six hundred pages, he has sought to exhibit every detail that can be traced bearing on the career and personality of Herbert. It is impossible to praise too highly the industrious research, the comprehensive erudition, and the felicity of style that have been brought to the construction of this noble monument. Signor Rossi's pages abound in matters of interest to students of the early seventeenth century, and historians will quarry in his chapters on Herbert's public activities. Volume I pursues in detail Edward's flamboyant progress from 1582, the year of his birth, to his appointment as ambassador at Paris in 1619. The narrative richly expands the bravado of the *Autobiography* and throws a flood of light on the instruction, the interests and the travels of a versatile nobleman of the period. When, in the second volume, the author reaches the story of the diplomatic mission in Paris, he gives a vivid representation, based on Herbert's letters and reports, of the complicated currents of international politics. The first clashes of the Thirty Years' War had occurred. Placed between the Protestant sentiments of his country and the tortuous negotiations of James I with France and Spain, Herbert's position was delicate. But he displayed capacity. He strove to intervene between the government of France and the Huguenots and he conveyed salutary warnings to his royal master in the matter of the Spanish marriage. Signor Rossi interprets the events which concluded in Herbert's sudden recall in a manner different from that given by Sir Sidney Lee in his edition of the *Autobiography*. All

these affairs are treated in graphic detail. But we cannot here dwell on the escapades and public activities of Herbert's early life, nor upon his conduct during the Civil War. The story of his later years, devoted largely to literary pursuits, is unfolded in the second and third volumes of this splendid work. Every source of information has been ransacked, every utterance of Herbert has been considered. To Signor Rossi's biography of the quixotic author of *De Veritate* is due the warm gratitude and respect of English scholars. It ought to be translated as soon as possible.

Signor Rossi affirms that no one has ever really studied the philosophy of Herbert and he finds an exasperating superficiality in all existing accounts. It is impossible to dispute his right to this opinion. But *De Veritate* is an extraordinarily perplexing work. Herbert was a dilettante, an Elizabethan scholar, a knight-errant of philosophy. The book is an eclectic tissue of theses, haphazardly arranged and presented in turgid Latin. And a nearer view discloses much incoherence and imprecision of thought. Nor can we discern in it any manifestations of the new notions that were stirring at the time of its composition. Its terms and principles are derived from the degenerate scholasticism of the day, and from the hermetic Neo-Platonism of the Renaissance. This loose amalgamation of doctrine is directed to several incompatible purposes. The ostensible aim of the book is to expound a theory of truth and to distinguish it from revelation, probability, possibility and error. But considerable sections are devoted to a phenomenology of knowledge and to an antiquated psychology of various forms of cognitive experience rather than to the justification and test of truth. In the face of these defects we may well inquire what value the work possesses. But in order to answer this question it will be necessary to glance at the theories expressed in it. Let us attempt, with the inestimable advantage of Signor Rossi's guidance, a brief survey of the philosophy of *De Veritate*.

Herbert opens with the assertion of a series of axioms relating to truth that seek to comprise and bring into harmony traditional definitions. The definitions are taken from a list given by St. Thomas Aquinas. There appears immediately in these axioms Herbert's main belief that truth is a *conformatio*. This conception reposes on a long history into which we need not here enter. Suffice it to say that Herbert oscillates between the Aristotelian-Thomist view that knowledge is the result of the operation of the active intellect on the particular phantasms of sense, and the Augustinian view that knowledge is a direct apprehension of universal realities. The opening discussions maintain both that truth is a relation and that it is an intuition. Yet both theories of truth are said to be "conformities"; truth is a conditional conformity between objects and our faculties and also a direct conformity between our minds and fundamental notions or ideas. And in the further allusions to the four forms of knowledge now distinguished, *Veritas rei*,

Veritas apparentiae, *Veritas conceptus*, and *Veritas intellectus*, Platonic, Aristotelian and Stoic conceptions are incongruously combined. *Veritas rei* is defined as truth of the thing as it is independently of all conditions (D.V. 1633, p. 9). Yet a number of conditions are at once attached to it. It must be in conformity with our cognitive capacity, it must be of a certain size (we cannot know atoms), it must possess *differentiae* or distinguishing marks, and it must be related to an inner faculty (D.V. pp. 13-15). The scholastic doctrine of *difference* is here important for the elucidation of Herbert's meaning. In his hands it becomes the doctrine that every individual thing possesses an inner mark (*terminus*) by which it is distinguished from every other individual; this mark is perceived directly by an "inner sense". Signor Rossi detects echoes of the famous *haecceitas* of Duns Scotus and of Ockham's insistence on particular knowledge. But the doctrine is intimately associated with a cardinal principle of *De Veritate*, the principle of *analogia*. Every individual being that we can apprehend possesses a secret analogy, or correspondence with some feature of our mind, for we are microcosms of the universe. Late scholastic theory unites with the hermetic and cabbalistic philosophy of the Renaissance. Every item is mirrored in its counterpart; affinities pervade all phenomena. "The principles of all the *differentiae* in the world are inscribed within man," and "the analogous faculties in us respond to every object". *Ex harmonice mundi systemate is detur rerum ordo ut objecta facultatibus et vice versa respondeant* (D.V. p. 127).

Veritas rei then, assumes a reciprocity between the ideal individual reality of every object and a special faculty of the mind, a faculty of the inner senses. *Veritas apparentiae* relates to the sensible images of things, the *species sensibilis* of the schoolmen. We need not dwell on the conditions that are pronounced to be required in order that appearances should be in conformity with our external faculties. The most important and the most confusing condition is that there must be a fitting medium, such as still air. The principle is extended to all forms of knowledge; they all require suitable media. The insistence on the elaborate scheme of conformity that follows from the three-fold adjustment of objects, faculties and media throughout the scale of knowledge awkwardly consorts with the emphatic declaration of direct intuitive knowledge that is the distinctive mark of *De Veritate*. When he is discussing *veritas apparentiae* Herbert describes knowledge as a process in which things as they are produce appearances (*ectypum seu forma vicaria rei*) which give rise to images in our minds; the simple concept (*veritas incomplexa intellectus*) is formed into a universal (*veritas complexae intellectus*) by the act of combining the simple concepts. But in other passages he maintains that truth is always a *sensus*, an intuitive consciousness of objects. Thus in one passage he asserts that universals are immediately known. When a thing affects

us in the same manner as other things a universal is apprehended. No process of comparison is involved, for we perceive directly the conformity that actually exists between items of reality (D.V. p. 29). The main doctrine of the work is undoubtedly that truth is the intuition of the manifold 'analogies' that unite every aspect of reality; but this Stoic and Hermetic position is associated incoherently with Aristotelian accounts of knowing, in which the stress falls upon the mind, compounding and dividing the appearances.

Passing over the list of conditions attached to *veritas conceptus*, or truth of perception, let us proceed to the chief doctrine, the doctrine concerning *veritas intellectus* and the Common Notions. Here we turn to the division of the faculties into four classes: natural instinct, internal sense, external sense and *discursus* or logical inquiry. And it is necessary to attempt to make clear some of Herbert's principle terms. We have seen that the basic theory is that every kind of cognition corresponds to an item in reality. The cognitive forms are named faculties. The term *facultas* is used in the sense of *virtus* or *potentia* of the schoolmen; it means a capacity, not a factor of the mind. Herbert speaks of innumerable faculties; there are as many faculties as there are objects. But he does not fracture the mind into an infinite number of faculties, as Descartes and Gassendi accused him of doing. He distinguishes clearly between the mind and its acts (cf. D.V. p. 31). He describes them as rays of the mind that pierce the apertures of the sense-organs and perceive the aspects of things to which they are related in accordance with their reciprocal analogy. Every act of apprehension is related to a feature in things. When a faculty is in conformity with its object a *sensus* occurs. All possible *sensus*, or apprehensions, are now grouped into the four classes that have been mentioned; and at this point we must notice an important division among the modes of analogy that pervade reality. There are three grand classes of analogy, that between the human mind and God, that between our minds and things, and that between things themselves. The classes of faculties and senses, natural instinct, internal sense, and external sense are concerned in order with the three analogies or correspondences just given; *discursus* is not concerned with any of these and is therefore the least valuable type of knowledge.

The supreme mode of knowledge is found in natural instinct. Here Herbert's studies of the fashionable philosophies of the previous age unite with classical theories of reality. The natural magic of Cornelius Agrippa, Paracelsus, Patrizi and Telesio is combined with Stoicism and Ciceronian teaching. The alchemists, the medical writers (particularly Severinus) and the *corpus hermeticum* contribute to the character of natural instinct. Especially the belief in an *anima mundi* and in the comprehensive analogy between man and the universe are present in Herbert's discussion. There is a fundamental tendency in all nature towards self-preservation and

this tendency appears in man as the desire for immortality and blessedness. It also appears in the form of innate notions or propositions, common to all normal men. Thus these common notions are a portion of the universal reason active throughout nature, tending to the preservation of individual things, of species and general classes and of the universe itself (D.V. p. 57). Common notions are conformities between natural instinct in man and its spiritual objects. The vital point in Herbert's doctrine is that the common notions express our practical being rather than our theoretical understanding. They are principles of action rather than cognitive principles. Not only are the basic propositions of religion and morality common notions but there is in them a close fusion of knowledge and will, of truth and value. Truth is founded on the common notions, but the common notions are founded on natural instinct, and natural instinct governs our analogy with God and with the world. Herbert gives a remarkably heterogeneous series of examples of common notions, elementary conceptions in religion and law, the idea of substance, the appreciation of beauty. He offers six marks by which they can be distinguished; they are assumed prior to reflection and experience, they cannot be derived from further principles, they are universally acknowledged, they are certain, they are necessary to our preservation and salvation, and they are in conformity with their objects, that is to say they are intuitively grasped (D.V. p. 60). Elsewhere much stress is laid on the testimony of universal consent.

The section on the common notions, 'the sacred principles which it is not lawful to dispute' is followed by a long description of the internal senses. We have seen that this group of apprehensions are concerned with the analogy of our minds with objects. Several divisions are made and discussed at length. There are those physical impulses that are analogous to the four elements; those feelings of pleasure or repulsion that respond to the essences of objects; and inclinations to moral ideas and behaviour. This latter group is no doubt partly derived from the *voüs* of Stoic thought, the activity that controls and brings to harmony the lower impulses. The general *sensus* of the inner senses is conscience, the function of which is to bring all the impulses of our minds into conformity with common notions (D.V. p. 125). Herbert, in fact, treats of feelings and impulses under his second class of senses, not of cognitions. The next part of his inquiry comprises a protracted and desultory exposition of the external senses. It is a series of disjointed references to the medical authorities and the Herbals. There is a protest against the traditional doctrine of the five senses in accordance with the author's favourite theory that there are multitudinous faculties responding to their proper objects (D.V. p. 129). But these pages serve only to display the author's disorganized erudition.

Since knowledge is an intuition of truths the rôle of the fourth

class of cognition, *discursus*, is secondary and otiose. Reason, in its work of combining and dividing, of drawing inferences from premises, is superfluous where a common notion is available or when the inner or outer *sensus* make pronouncements; *super-vacanea est facultas ista* (*D.V.* p. 151). Herbert pours contempt on its activities; it is the source of error and folly. Yet he says some puzzling things about it. On the one hand it cannot arrive at any new knowledge, since it does not discern the inner nor the outer analogy of things, nor the common notions. On the other hand it is said to be concerned with the analogy between things and, with the aid of the common notions, to be occupied in connecting them (*D.V.* p. 154); and this gives important fresh knowledge. But discursive reason pursues its task of seeking the analogies between analogies perceived by the other senses in an external way that is subject to many rules and conditions; and its liability to error is associated with man's freedom to sin. The brief passage on *discursus* leads at once to the *Zetetica*, the new logic which has been confidently announced at many points of the preceding sections. Now this logic, 'the key to every doctrine of law', concerns the proper employment of *discursus*, that is to say the discernment of the right relations between the analogies perceived by the various senses. Its work is to distinguish the correct *sensus* and type of faculty that must be applied to each form of relation in order to reach conformity. There is now displayed a strange adaptation of the Aristotelian categories. The eleven questions, *an?* *quid?* *quale?* and the rest seek to classify all the mental acts or faculties that can be brought to bear upon objects. They raise the question, What faculty gives knowledge of quantity, what of relation, what of time? The *Zetetica* attempt to be a canon of rules for deciding what type of faculty is competent to deal with every aspect of an object. A table of results is presented. We read, for instance, that the question *quando?* depends for answer upon the internal and external sense. But Herbert completely fails to preserve a clear distinction between his questions, and the discussion of each question rambles freely and superficially over many topics.

Numerous currents of thought mingle in disorderly confusion in the rhetoric of *De Veritate*. Yet in this very confusion dwells part of the interest of the work. Nowhere else in this age can we find a more remarkable instance of the eclecticism, the almost desperate eclecticism of conservative speculation at the opening of the seventeenth century. When we seek to distinguish some of the strands of Herbert's conceptions and to trace the source of his ideas, we reach an acquaintance with large tracts of the intellectual culture of this critical time. Here is the basic Aristotelian instruction of the Universities, the Aristotle of the commentators, of Suarez. Here also are fragments of the great polemic against Aristotle that had sprung up in the latter part of the sixteenth century. The theses of the *Summa Theologica* and the *Quaestiones*

Disputatae of St. Thomas appear in combination with the doctrines of Duns Scotus. Neo-Platonic theories, the substantiality of aesthetic ideas, for example, are present in shapes transmitted by St. Augustine. But there are two influences that dominate Herbert's speculation. One is the classical humanism of the Renaissance, the philosophy of which was principally Stoic. The belief in innate ideas, in the correspondence of truths and beings, in the universal reason of the cosmos, are taken from Stoic thought; above all the conception of common notions is due to the master of humanism, Cicero, and to his Stoic authorities. The other important source of Herbert's ideas is the tradition of occult theory transmitted by the Italian Platonists, the natural philosophers, and the alchemists from the *corpus hermeticum*. The doctrines of secret analogies and signatures controls the central principles of *De Veritate*. All these elements in the strange mosaic are distinguished with masterly care by Signor Rossi. But Herbert's book is representative in a further way. It expresses the profound intellectual dissatisfaction that governed men's minds after the Elizabethan age. Scholars who were out of touch with the new developments in natural theory were searching the authorities for guidance; and Herbert is not alone among the erudite courtiers of the period who found salvation in distrust of reason, in a philosophy of authoritative intuition.

But the subsequent notoriety of *De Veritate* was not due to its theory of knowledge; it was due to the application of the theory in the final section of the book to the common notions of religion. Here are presented the celebrated five notions, 'the true Catholic church which never fades' (D.V. p. 210) that became the ground-plan of Deism. But Herbert's deism is not that of the eighteenth century. It is true that the deists appealed frequently to the common notions of morality and religion with which normal persons are endowed at all periods. Through Charles Blunt and Charles Gildon, Herbert's principles descended to Toland, Collins and the rest. But the deists resorted to evidences of reason that Lord Herbert could not have accepted, for, as we have seen, in his view of knowledge discursive reason is severely depreciated. Moreover he recognizes, in a limited way, the place of cult and ceremony in religion. But especially his natural theology is divided from later deism by the acceptance of revelation. Revelation occupies, however, a peculiar position in Herbert's scheme. It is sharply separated from the plan of truth expounded in the body of the treatise, the doctrine of the conformity of faculties and objects. It is classed with appearance or probability (*verisimile*) and with possibility as one of a radically different group of truths. The mark of these notions is not genuine truth but external authority. Revelation depends on the authority of God, probability which is knowledge of the past on the authority of historians, possibility which is knowledge of the future on the authority of prophets. A series of tests by which a genuine revelation may be distinguished from a false one are

offered ; but since it is a form of knowledge that is given only to certain persons endowed with special faculties it fails to exhibit the essential character of truth, that is to say, universality. Yet even here Herbert contradicts himself and speaks of a supernatural sense (*D.V.* p. 228).

There is in fact throughout the exposition of the common notions of religion and of revelation a constant tension between general and particular providence, between conscience and grace, between natural religion and supernatural faith. In most of the discussion, Herbert leans towards a view of religion that is independent of all authority save the laws written upon the hearts of all men, the religion of Cicero's *De natura deorum*. The contemporary revolt of the extreme Protestant sects against the priests breaks out, and we are told that the divine Judge will demand on the last day an account of each man's deeds not according to a faith received from outside but according to his own (*D.V.* p. 209). The Common Notions are sufficient ; no further revelation is required. But Herbert constantly draws back from pure deism and declares his belief in the gifts of grace, in special revelations attested by signs and wonders. The contradictions provoked by these diverse tendencies are endless and Signor Rossi misses none of them. But Herbert moved steadily in the direction of a self-sufficient, pre-Christian theory of religion, and in *De Religione Gentilium* he reaches the conclusion that the faith of the pagans was more acceptable than Christianity. For the subsequent history of his attempt to laicize religion, students of philosophical theology must be invited to read the excellent survey in Chapter VII, Vol. III, of Signor Rossi's book. And it will be a duty of all future historians of philosophy to peruse his examination of Locke's misconceptions concerning Herbert's Common Notions in the first book of the *Essay*.

This magnificent biography concludes with an extensive list of books and with thirty-five appendices touching many points of Herbert's activities and writings. Here, as in some towering mausoleum of the Renaissance, is enshrined the chequered career and the diffuse learning of Lord Herbert of Cherbury ; here, for the first time, is portrayed the manifold argument of *De Veritate*.

M. H. CARRÉ.

The Physical Background of Perception. By E. D. ADRIAN, O.M., F.R.S. Being the Waynflete Lectures delivered in the College of St. Mary Magdalen, Oxford in Hilary Term, 1946. Oxford : At the Clarendon Press (Geoffrey Cumberlege), 1947. Pp. 95. English price 10s. net.

IT was to be expected that the central nervous system would only begin to yield its secrets at the point where its activities became accessible to controlled observation and experiment. That is why

we find the physiologists with still very little to say about the activities of the *units* of the nervous system—the nerve cells, and a great deal to say about the activity of its vast network of connecting threads—the nerve fibres. For while the actual nature of nervous impulse and its genesis in the cell body are not yet understood, it was known before the turn of the century that the *transmission* of impulses through nerve fibre is invariably accompanied by an observable electrical change.

It is largely through the labours of Professor Adrian that we in this country have come to know of this propitious circumstance, and of the possibilities of breaching the defences of the central nervous system which its discovery immediately opened up. In the book before us we have renewed proof of the debt owed by the modern student of nerve and brain to the electro-physiologist. The argument is anchored at every point to the systematic observation of galvanometer readings, augmented (since the war of 1914) by valve amplification, of minute changes of electrical potential in nerve-fibre through which impulses are passing. These changes register the occurrence, if they do not reveal the actual nature, of the 'messages' which nerve-cells are signalling to each other and they are the source of "most of our information about the events which take place in the nerves and the brain" (p. 10).

Certain basic uniformities of nervous transmission were immediately inferable from the character of its electrical effect. It appeared, for instance, that impulses in the nerve-fibre could not vary in intensity. Increase of intensity in the stimulus is transmitted as increase of frequency in the incidence of impulses, an increase, that is, in the number of distinct impulses which pass in unit-time. But the most important lesson of the galvanometer readings was that the 'messages' from different kinds of sense-organ or different parts of the brain are all of the same simple pattern. "Records of the electrical activity of the nerves leave no doubt that there is only one kind of change which can be conducted down a nerve-fibre, only one kind of impulse" (p. 13). This means that auditory sense-quality differs, for instance, from visual "because a different part of the brain receives the message and not because the message has a different form" (p. 14).

Adrian's procedure in these lectures is to begin by making clear that the connective network of the nervous system thus operates on a "simple and uniform plan". He thinks that, in view of this peculiarity, "the activity of the brain from moment to moment should be capable of definition as a spatial arrangement and no more" (p. 15). And, following this clue, he puts forward as his principal thesis the view that perception can be understood in terms of the construction by the sense-organs of an extended reproduction or 'map' of the external world in the percipient's brain. Lecture I closes on the note that the latter is "large enough to have the external world mapped out on it by our sense-organs"

(p. 16), and in the lectures which follow the mechanisms of sense-reception are described with a view to showing how the brain could have such a "map of our surroundings" formed within it.

In a study of the mechanisms of perception, the representational or 'sensory' signals naturally occupy the centre of the stage. (With regard to the efferent signals from the 'motor-area' the only point Adrian develops is that duplicates of these go to the cerebellum, the automatic timing mechanism which ensures the smooth functioning of whatever executive apparatus is brought into play.) Of the sensory signals, a group coming from receptor organs in the muscles, tendons and joints (Sherrington's 'proprioceptors') serve the automatic mechanism which controls posture and locomotion. They provide 'information' which, because it is not registered in consciousness, cannot affect the content of our mind but is utilised, as Adrian puts it, in the "detailed administration of the body". Any 'map' which may be elaborated at this level has a purely 'intrasomatic' significance. Still largely involved in the automatisms of movement and posture are the 'messages' from receptor organs in the skin (Sherrington's 'extroceptors') which inform the organism of changes in its immediate environment. The greater part of Lectures II and III is taken up with the discussion of the cortical 'receiving area' for these messages from the depth and surface of the organism, and of the receptor organs for this area, particularly (in Lecture II) the muscle-spindles which supply information "in the light of which the force of muscular effort can be adjusted to the load" (p. 21). Special mention is made of the 'pain-signal' which is closely associated with these intrasomatic and tactile groups. Its electrical concomitant is disproportionately small to the intensity of the sensation felt. This is because precise *localisation* of a pain is unnecessary and the conducting fibres can, accordingly, be "of the small, slowly conducting type which give very small electric effects" (p. 28). On the other hand, the *conscious* signal must be vivid and have priority over all others because it reports a situation to which the organism cannot afford to remain inattentive.

In contrast with the centres involved in the constitutional and executive administration of the body, the cerebrum or 'roof-brain' is concerned "with long-range objectives and with behaviour related to the outside world" (p. 42). And the greater relevance for this purpose of the tactile information received from the exploratory snout in animals, and from the hands and fingers of human beings, is reflected in the greater *extent* of the cortical receiving areas for these areas of the skin. The meaning of this is that, in proportion to what might be called its greater 'trans-somatic' significance, the map of events at the surface of the body is presented here "on a much more generous scale" (p. 42).

Of supreme significance to the cerebrum, however, in its direction of 'long-range' behaviour is the information which comes from

what Sherrington appropriately called the 'distance-receptors'. The most important of these are the organs of sight and hearing which form the subject of Adrian's fourth lecture. The 'map' elaborated by these organs has much more to do with guiding intelligent behaviour than the type of sensory information that has so far been discussed. We do not *think in terms* of 'depth' and 'surface' signals as we do in terms of sight and sound. In connexion with sight Adrian emphasises the *extent* of the cortical map for the centre of the retina, the area with the greatest concentration of the discriminatory 'cone' receptors. It appears that here the proportional size of 'receiving' to 'receptor' area is of the order of 10,000 to 1. The visual messages are clearly the most important of all those reaching the human brain and, on the assumption that the size of a cortical map is in direct proportion to its usefulness to the cerebrum, Adrian concludes that the purpose of this great magnification is to give the visual pattern "much easier access to the mind than any other of the sensory patterns" (p. 59).

"It is the existence of an order and a fixed relation between points on the body and points on the cerebral cortex which justifies the idea of a mapping of sensory events in the brain" (p. 37). Adrian gets one of his clearest cases of this correlation in the established isomorphic correspondence of the auditory receptor to the auditory receiving area. "The auditory messages come from the basilar membrane in the cochlea. . . . Owing to progressive changes in the length, tension, and loading of [the] transverse fibres [of this membrane] the frequency of the sound waves will determine the position along it where the vibration will be most intense" (p. 49). Experiments on the dog have shown that "there is a continuous strip of cortex . . . to which the different parts of the basilar membrane are connected in order. . . . Notes of high frequency produce activity in the front part of the strip and notes of low frequency behind, and the successive octaves are represented at approximately equal intervals of 2 mm. along the cortex" (p. 50).

Such facts as these do indeed strongly suggest the view that perception is somehow based on an extended physiological representation of external objects and events. But the solution of the problem of perception as a *mental* activity seems to me to be as far off as ever. Adrian cannot claim to have established more than that a 'cortical map' occurs. But the occurrence of a map is surely not equivalent to a perception of one. A map is not necessarily perceived because it occurs, however true it may be that it could not occur *as a map* if it were not perceived. That the pattern of nervous excitations should be a 'map' presupposes that it is an object of which there is awareness, and this awareness, by which the map is read, can hardly be identical with the map itself become self-conscious! Adrian nowhere explicitly identifies the map with consciousness. He does imply that, in order to achieve a sensory

or informative significance, the cortical pattern must "rise into consciousness". But, at the same time, he speaks of a complete *sensory* map as "formed in the brain"! (p. 43, my italics). In any case there is the further difficulty, even if Adrian did suppose an awareness of the cortical map, that perception, while it may occur by virtue of a complex pattern of nervous excitations, is most decidedly not a perception of this pattern but of the surrounding world, or at least of what is ostensibly an order of physical objects and events. Nothing Adrian has to say meets what I take to be the crucial difficulty, *viz.*, that of showing how the construction of a map of objects and events in the brain becomes a perception of these objects and events themselves.

We are told in the fifth lecture of a curious feature of the electrical activity of the *brain*. This is the 10-a-second rhythm, or Berger's α rhythm, a regular cortical 'beat' which, there is detailed evidence to show, is somehow associated with the state of drowsiness or inattention. The decision to attend and the direction of attention, in which philosophers have often supposed they had found the essence of subjective activity, has as its condition, it now appears, the "suppression of the uniform beat of the α rhythm" (p. 77). And it confirms the suspicion that attention-process was, in some way, fundamental that physiologists agree in locating somewhere in the 'central regions of the brain' the *decision* whether or not the α rhythm is to be present and what part of the cortex is to be set free from it for the use of the mind.

Two questions of perennial concern to philosophers are raised in the sixth (and last) lecture. (a) What mechanism conditions recognition and the abstraction of repeated patterns which makes recognition possible? In particular, in what form is an abstracted pattern, for instance, the notion of triangularity, *retained*? "Where", in short, "are memory traces stored, and of what are they made"? (pp. 88-89). (b) At what point in the chain of physiological causation do mental events come in? Adrian does not himself answer these questions but relies on hints thrown out by other people. He mentions the conclusion, to which Lashley's researches seem to point, that memory traces are not stored in *any* particular part of the brain, and cites this psychologist's suggestion that they may be in the nature of interference or resonance patterns "reduplicated all over the cortex" as "characteristic patterns of activity in the local circuits of the nerve cells" (p. 91). Incoming signals, on this hypothesis, if they fit into the pattern, "find the resonating systems ready to be stirred up to increased activity" (*ibid.*).

In answer to the second question a view of Kenneth Craik's is cited. According to this, thinking depends on the operation of various nervous mechanisms which produce symbolic models of physical reality *similar* to the mechanical devices employed to calculate, for example, whether a bridge of a certain construction

would stand or collapse. Images and thoughts emerge at points where the physiological patterns have acquired a particular kind of definiteness, and are thus to be regarded as the finished product of an elaborate machine. This, we are assured, does not reduce thinking to an otiose accretion. For problems are only solved when thinking occurs, that is, when the symbolic cortical patterns "acquire the necessary brilliance and definition" (p. 94). But this is suspiciously like saying that thinking is the map (or model) become conscious! And in any case, there is nothing particularly novel about Craik's suggestion. It was a Gifford Lecturer and a Hegelian Idealist who said that consciousness comes "on top of immense stores of unconscious mechanism and adaptation" and is "a light and a revelation which comes only when it is prepared for and demanded" (Bosanquet, *The Principle of Individuality*, etc., p. 219).

We cannot complain, however, that our most pressing problems thus remain unsolved. Adrian warns his readers at the outset that the impression likely to be left by his book is of "the complete inability of contemporary science to give a satisfactory picture of any kind of mental activity" (p. 1). This note of agnosticism is disappointing but unfortunately necessary. It should, however, effectively silence the claim, still obstinately made in some quarters, that philosophers can no longer raise questions which the physicist and brain physiologist between them cannot answer.

J. R. JONES.

X.—NEW BOOKS.

The Approach to Metaphysics. By E. W. TOMLIN. Pp. xii + 276. London, Kegan Paul, Trench, Truber and Co. Ltd. 1947. Price 12s. 6d. net.

MR. TOMLIN tells us in his preface that this book is "neither a chronological history of philosophy nor the exposition of a complete system of thought; it is an essay in philosophical tendencies designed to throw light upon our present perplexities, and directed towards the removal of certain misconceptions about the purpose of philosophical enquiry and the relation of such enquiry to the problems of civilised life." We are also told that it is written to encourage the general reader to study the actual works of the great philosophers. The success of the book should obviously be judged in the first instance by how far it attains this aim. It is written in a fresh and vigorous style, enlivened at times by epigram, and the general reader might well be carried by the force of the author's conviction to read further in the originals. Whether he will get a very clear idea of what is the philosophical enquiry which Mr. Tomlin believes to be so important is another question. For Mr. Tomlin seems to have two views which alternate in different parts of the book. According to one, which he acknowledges he has learnt from Collingwood, metaphysics is "an historical study of presuppositions", of the assumptions lying behind science and civilised life. The other view is a *philosophia perennis* of a Platonic type, in which philosophy is a study of "ultimate reality", as distinct from appearance, and its method is one in which the mind turns inward upon itself. The contrast is set already by two quotations on the title page, one from Croce and one from Boethius.

Bots Collingwood and Mr. Tomlin launch their philippics against logical empiricists as traitors to the cause of philosophical (here identified with metaphysical) enquiry. But if metaphysics is a purely *historical* enquiry into the presuppositions which have in fact been made in different periods, one wonders whether there is anything in this to which the purest empiricist need object. Neither Collingwood nor his disciple Mr. Tomlin ask this question. Perhaps this is because they do not really believe that the historical enquiry is the whole of the story. They both want to claim that these presuppositions, when discovered, are articles of faith which we must go on holding if science and civilisation are to continue. Hence perhaps, and less consistently, Mr. Tomlin's Platonic motif. The only one of these articles of faith which emerges from his discussion is the belief that God is a creator God, responsible for both mind and nature, thereby securing "the presupposition of all science" that "there is a world of nature which obeys rational and uniform laws". (But how many questions does this beg?) Mr. Tomlin seems less interested than was Collingwood in the possibility of a shift of presuppositions even within the movement of science and civilised thought.

Mr. Tomlin acknowledges three dominant influences: Collingwood, A. E. Taylor and Whitehead. The view of metaphysics as the historical study of presuppositions is his heritage from the first of these; the view of it as the study of ultimate reality, "a form of thought exempt from material limitations" may have been encouraged by his studies in Taylor in his more Platonic vein. Whitehead can hardly be accommodated to

either of these views ; his view of metaphysics as a series of attempts to arrive by "descriptive generalisation" at general cosmological ideas is realistic in intention and cannot in fact be squared with Collingwood's view, in spite of verbal resemblances in certain phrases which Mr. Tomlin quotes, mainly from *Adventures of Ideas*. (Incidentally, like many other people, he misquotes Whitehead's saying that "Religion is what the individual does with his solitariness", calling it Whitehead's "unfortunate definition". It is clear, however, in the context that it is not intended as a definition, but as an aphorism, supplemented by other sayings bringing out other facets of the subject). Moreover, since Whitehead expressly tells us that his God is not a creator God, he cannot be roped in so easily as an upholder of this "presupposition of all science" (p. 264).

The survey of some of the main features of the history of European philosophy is written in a lively way and may well make the common reader feel that the great philosophers were talking about interesting things which would repay closer study. Mr. Tomlin is not, however, always a safe guide. For instance, the Ontological Argument is stated as that "a perfect being must be credited with the quality of existence because a perfect being must necessarily possess every quality" (p. 146 n.). Of Descartes, we are told that he allows God alone to be Substance (p. 165). Locke's Ideas of Reflection are confused with Complex Ideas, and are described as "ideas manufactured by the mind itself from the material gathered" (in Ideas of Sensation). He overlooks that they are for Locke "that notice which the mind takes of its own operations". Perhaps more serious, since more is made to hang on it, is the statement that Aristotle "represented God's activity as wholly capricious". This is surely an extraordinary misunderstanding of the meaning of *actus purus*. Aristotle repeatedly says that neither *ποίησις* nor *πρᾶξις* is compatible with the divine perfection, which is *γεωρία*, and *θεωρία* which can only be of the "best", i.e. of its own nature. This charge of capriciousness is levelled at strangely different people: at any in fact to whom thinking is not by its own nature "autonomous". So mental activity is called capricious in the views of Locke and Berkeley; in Descartes, because he sought a method for arriving at truth and contrasted it with the mind's own liability to error; in the mediævals because they held that thinking needed logical discipline; and in realists who try to discover order in objects external to the mind itself. But all these people were concerned with disciplining the mind against error because they were in their various ways concerned with trying to see the conditions within which it could arrive at truth. It would be tempting, but not altogether fair, to suggest to Mr. Tomlin that it is his attitude and that of Collingwood which is the more properly to be described as "capricious": tempting, because they bring us finally to presuppositions which are neither true nor false, but which we must decide to hold; but not fair, because they would say that thinking has its own method internal to itself. Perhaps this is why what we are told is an historical inquiry can also be an inquiry in which the mind turns inward and discovers what sometimes sound rather like innate ideas. I suggest this as the possible link between Mr. Tomlin's historical relativism and his Platonism. But it is a strange kind of history.

I have noticed two misprints: on page 97, "écraser l'inflame" and on page 132, l. 18: "world" for "word". On page 156, l. 28, "central" should perhaps be "centre of".

DOROTHY M. EMMET.

The Missing Factor in Science. Inaugural Lecture by Herbert Dingle, Professor of History and Philosophy of Science, University College, London. London, H. K. Lewis & Co., Ltd., 1947. Pp. ii + 18. 2s.

THE belief current in the nineteenth century that philosophy competes with science in discovering truths about matters of fact is dead: the belief that eminent scientists are qualified to pontificate about metaphysics in their leisure moments is moribund. Certainly both are better out of the way, but their disappearance has created a problem as well as an opportunity. Most philosophers nowadays are conscious that they would be better philosophers if they knew some science, and many scientists recognise that philosophy is not an alien subject but is intimately connected with scientific enquiry. There is thus a general conviction that what is vaguely called 'the philosophy of science' is important: but there is little agreement as to what it is and how it should be taught.

Prof. Dingle in his Inaugural Lecture has faced these questions and proposed an answer. Naturally his approach is from the side of the scientist, but philosophers will find much to approve and something to criticise in what he has to say. The task he sets himself is 'to enquire how it has come about that a generation so amazingly proficient in the practice of science can be so amazingly impotent in the understanding of it'. His contention is that 'the state of unselfconscious automatism in which science finds itself to-day is due to the lack throughout its history of a critical school, working within the scientific movement itself and performing the function which criticism has performed for literature from the earliest times'.

This is a promising line of approach. Prof. Dingle is easily able to show by quotations from the works of Sir Arthur Eddington and Prof. Bernal that highly qualified scientists sometimes talk foolishly about the nature and purpose of science; and those who remember the saddening analysis performed by Prof. Stebbing in *Philosophy and the Physicists* will admit that he would have found no difficulty in multiplying instances. It is, however, the remedy prescribed for this complaint in which we are mainly interested, and, although Prof. Dingle's prescription is useful, I am not confident that it is adequate. He makes the following claims:

1. Nobody can talk sensibly about the nature of science unless he has an intimate knowledge of the history of science; the history of science is inseparable from science itself.
2. Nobody but a scientist can possess this knowledge.
3. The kind of criticism to which such study of science leads is valuable in two ways, 'it can direct the movement itself so that blind alleys are avoided and the path of progress illuminated, and it can make science self-conscious and aware of the significance of what it is doing in relation to other human activities'.
4. Such criticism is generally disregarded by recognised scientific bodies, such as the Royal Society. These attend only to concrete new achievements. Hence the philosopher scientist will be 'like Bunyan's Interpreter, he makes pilgrimage possible and is heard of no more'.

This conception of the philosophy of science is interesting and important. My criticism of it is that it is oversimplified. What Prof. Dingle has done (and it would be unfair to blame him for over-compression in a single lecture) is to combine two different lines of criticism which it is

necessary to distinguish if the issue is to be clearly recognised. There are two ways in which scientists can be 'unphilosophical':

1. They may fail to see the wood for the trees. This is strictly a defect within science which Prof. Dingle's prescription is well-calculated to remedy, or at least to mitigate.
2. They may hold and propagate misleading and even nonsensical views about the nature and purpose of scientific enquiry in general. But such tendencies will at best only be curbed by a better acquaintance with scientific history. What is needed to eradicate them is surely greater competence in logical analysis.

The point of this distinction is clear if we revert to the case of Sir Arthur Eddington. There is no reason why he should not himself have eventually dissipated the obscurity which covers his own later work on the fundamentals of physics (which is a task assigned by Prof. Dingle to the philosopher scientist). He might have done this and still talked nonsense about his 'two writing desks'. Or he might have been converted by Prof. Stebbing to a saner theory of knowledge but have remained hopelessly obscure about the fundamentals of physics. There are in fact two higher order activities required here, one within science and the other about science. Prof. Dingle rightly criticises Eddington and Bernal for doing the second badly, but his remedial instructions are confined to ensuring that the first is done well.

I have laboured this point because it would be a great pity if Prof. Dingle and those who agree with him that philosophical training is genuinely needed by scientists were to omit an essential tool from the philosophical outfit which they provide. Admittedly some scientists (like other human beings) do not look critically enough at their own achievements or scrutinise with sufficient care the assumptions on which they work: and certainly the presence of qualified critics within the sciences could do much to help in the discarding of what is worthless and the promotion of what is valuable in contemporary work. That is what critics are for. But I submit that the critics will at best do only half their job unless, in addition to their acquaintance with scientific history, they have far more than a superficial knowledge of the nature of criticism itself; this involves the kind of thinking which is specifically logical and analytical, not historical in character. It is the function of professional philosophers to deal with this aspect of the problem, and they should be called on to do so.

T. D. WELDON.

Causation and the Foundations of Science. By J. O. WISDOM. Paris, Hermann et Cie. Pp. 54. Not priced.

BASICALLY this monograph is an attempt to "justify" induction as a rational activity by what Mr. Wisdom evidently considers to be the only possible method—a rationalist theory of causality. Other problems concerning the notion of cause—such as how it fits in with the theological doctrine of Creation—are early dismissed as unreal; even the problem of free will is regarded as illusory, and Mr. Wisdom dismisses it with the tantalising hint "It is possible with modern psychological knowledge to

give a detailed definition of 'responsibility' in terms of mental processes that presuppose determinism of the mind". It is to be hoped that Mr. Wisdom will tell us more of this.

The main question is set by Mr. Wisdom as follows. He shows that induction requires no more than a *de facto* regularity—that for practical purposes a phenomenalist analysis of science, which treats the formulæ of mathematical physics merely as unmeaning mechanisms for producing predictions, gives an adequate account of what goes on. But though this is satisfactory for the scientist, the philosopher must ask (a) whether the observed regularities are "due to causal necessity" (p. 6), or are mere brute facts; (b) whether without a rational concept of Cause the Universe must be pronounced to be basically irrational and hence incapable of scientific study. Mr. Wisdom claims that though the non-causal quantitative *scientific laws* produced by scientists are themselves to be interpreted phenomenalisti, they are in fact derived from qualitative causal laws which are genuine *natural laws*.

The reasons for which Mr. Wisdom regards a rationalist interpretation of causation as necessary are the same as the well-known reasons advanced by Dr. Ewing. He dismisses, however, the view that causation is a logically necessary relation, since there seems to be little reason to believe that we could ever discover causes purely deductively and instead advances a view which demands as an ingredient in causation what he calls 'physical necessity'. To summarise Mr. Wisdom's already overbrief account of this objective necessity: oxygen, for example, is a configuration of microscopic (submicroscopic?) parts; so is hydrogen. When we combine the two we have—must have—a new configuration. The first of these configurations microcentrally causes the empirical oxygen percepts, the second, hydrogen percepts; the combination microcentrally causes the empirical percept of water. Thus the objective necessity involves nothing more mysterious than the notion of two configurations of microscopic particles combining to form a new configuration, in Mr. Wisdom's opinion. He then says "While therefore it may be claimed for this analysis that it solves the philosophic controversy between exponents of *de facto* uniformity and logical connexion, so far as this is concerned with physical interaction of events, it does so only at the cost of laying the burden on another problem, that of Microcentric Causation, or that of emergent properties, but in doing this *it solves a philosophic problem by opening a scientific one and transplants the issue out of the domain of philosophy*." But unless I have quite misunderstood Mr. Wisdom, I think that the Humean question "But can we know rationally that the new microscopic configuration will always lead to the same empirical percepts?" will surely reopen the philosophical problem again.

I think Mr. Wisdom's monograph is a gallant failure to carry out an impossible task. I wish he would ask himself again what he really wants; if induction on a minimum interpretation gives us, as Mr. Wisdom says it does, the ability to control the world and make predictions, what more does he really want in order to feel comfortable about relying on it.

During the course of his argument some good and interesting points are made—for example his interesting sections on the different senses in which a prediction may be said to be uncertain. But I note with surprise that throughout he describes Hume's views in some such way as "Now common sense feels that Hume must be wrong and that the fire obviously does cause the paper to burn" (p. 30). Would Hume ever

deny such a thing? And, finally, what on earth does Mr. Wisdom mean when he says "If the Frequency Theory could be established, the problem of induction would be solved"? Surely the frequency theory of probability is a theory about the meaning of the word "probable", not an attempt to justify probable inference.

J. O. URMSON.

La Conscience de la Liberté. Par JEAN LAPORTE, Professeur en Sorbonne. Paris, Flammarion : Bibliothèque de Philosophie scientifique, 1947. Pp. 297.

PROF. LAPORTE is concerned to demonstrate the existence of free-will, and to re-establish the traditional notion of the 'free agent'. His method of attacking the problem he explains in his introduction as being that of self-examination: if free-will amounts to anything, he argues, it must be something which we are directly aware of possessing—"C'est donc la conscience qu'il faut interroger" (p. 19). Those determinists who deny the validity of this method he accuses of neglecting Descartes' rule that one should first ask of anything, not *an sit*, but *quid sit*. And further, he argues, if we are directly aware of possessing free-will, nothing else is needed to prove its existence: it does not make sense to ask a man who says that he is aware of possessing free-will how he knows that it is *that* of which he is aware—"La conscience, en tant que révélation immédiate de mon être à lui-même, est infaillible en droit" (p. 16).

The argument is presented in five stages. In the first ('Le déterminisme'), Prof. Laporte argues that neither science nor metaphysics can require us to accept universal determinism as anything more than an "acte de foi" (p. 28); that the laws of psychology are so vague and unreliable in application as to provide evidence of the breakdown of determinism in this field of study (his choice of example is, however, unfortunate—surely "la loi de reproduction du semblable : 'tout besoin tend à répéter la conduite qui lui a réussi précédemment, dans une circonstance semblable'" (p. 50) is as much an analysis of our notion of a 'need' as it is an 'explanatory hypothesis'); and that there is therefore scope for free-will (p. 62). In the second part ('La spontanéité'), he runs over the standard definitions of 'desire', 'habit', and 'instinct', and concludes that, although all three may be involved in a voluntary action, the notion of the 'will' cannot be exhaustively analysed in terms of them: the crucial element of decision, the 'effort of will', evades any such analysis (p. 119). Turning in the next part to 'L'effort', he narrows down the field of discussion by ruling out of account muscular effort, except in so far as it involves attention, and attention except in so far as it involves reflexion (p. 160). Only on the level of reflexion does the 'I' seem to him sufficiently 'concrete and alive' to be capable of free choice: "Or ce *je* nous élève à un étage nouveau et supérieur de notre être. Avec le désir, l'instinct et l'habitude, voire avec l'effort musculaire et l'attention spontanée, nous avions affaire à un *moi*, mais qui demeurait engagé dans la nature. Avec le *je* concret et vivant de la réflexion, nous avons atteint la personnalité et l'esprit. Et du même coup—on va le voir—nous avons trouvé la liberté" (p. 181). Part IV ('Le libre arbitre') is an attempt to reconcile the different features of 'free action'—'judgment', 'choice' and 'causation'—within the dogmatic framework of traditional logic ("Tout jugement est adhésion à

un *prédictat*. Et la *prédictat*, en dépit des apparences, comprend toujours deux termes : ce qu'on affirme et ce dont on l'affirme" ; p. 198). Part V is a discussion of the scope and limitations of our feelings of moral responsibility. Finally, in his Conclusion, the author presents us with the picture, familiar since Aristotle, of Man as the Captain of a Ship—"A lui seulement de choisir . . . en marquant son approbation d'un hoche-ment de tête aussitôt obéi" (p. 295).

Prof. Laporte is certainly on solid ground in directing our attention where he does. The notion of the 'will', as he very properly insists, is logically independent of electrons, of desire, habit, instinct, muscular strains, and all the rest. As he suggests, the kernel of the problem lies in the fact that in some situations we learn to say, "I made up my mind to do it ; so I did it", and are not prepared to consider anything which anyone else says to us as counting in any way against that declaration. Any final treatment of the subject must aim at bringing out the features characteristic of these situations, marking them off from those in which we say instead, "I couldn't help it" or "I don't know how it happened". This the author appears to recognise—"Nous ne risquerons pas de nous abuser quant à notre sentiment de liberté, si nous savons marquer exactement en quelles circonstances et à propos de quoi nous l'éprouvons" (pp. 283-284). Unfortunately, it is just at this point that the weaknesses of his approach begin to be important. He 'explains' the incorrigibility of the statement "I made up my mind, etc." by some questionable juggling with the word 'être'—"Cette connaissance . . . est vraie, d'après l'autentique définition de la vérité, celle d'Aristote et de Descartes—'la vérité c'est ce qui est'—puisqu'elle coïncide avec l'être, avec mon être" (p. 16); his characterisation of the 'will' does no more than emphasise the differences between 'will', 'desire' and so forth ; and for the rest we are left to battle with a mysterious 'moi intérieur' and an equally mysterious 'je'—"immatériel" but at the same time 'concret'.

Perhaps it is unfair to expect anything else. Until the limitations of the traditional logical forms are more widely appreciated, people will go on trying to analyse "I decided to do so-and so" in Subject-Predicate form ; and Prof. Laporte is not to be blamed for the inconclusiveness of the result. At any rate he has given us, within these limitations, an honest and acute discussion of a fascinating subject.

STEPHEN TOULMIN.

Intuition et religion. Le problème existentialiste. By PAUL ORTEGAT. Louvain : Institut supérieur de philosophie ; Paris, J. Vrin, 1947. Pp. 248. Price 110 fr. belges.

THIS book might well be described as an essay in religious epistemology. The first part of it consists of an historical survey of the sources of modern Intuitionism, and a straightforward exposition of its chief philosophical and religious tenets (pp. 7-103). In the second part, these doctrines are submitted point by point to a critical examination (pp. 104-209). In the third, the author expounds his own theory of personal realism, and tries to show that all that is valuable in Intuitionism can be incorporated into an orthodox Catholic philosophy (pp. 210-246). The treatment of these delicate matters is so liberal and reverent that the book may be recommended to every class of reader. It is not through any defect of

learning, logic, or fairness that it fails to establish itself as an authoritative treatment of the subject. That failure must be attributed to limitations that the author has imposed upon himself. Whatever view one may take of mystical experience, no treatment can profess to be adequate which does not deal fully with its physiological substrate, and bring to its psychological analysis something of that thankless lucidity which Bergson and Grégoire, each in his own way, has brought to this difficult subject.

It would be unprofitable to follow out in any detail the articulation of an argument so clear in its general design, and yet so complicated, and sometimes so repetitive, in its development. A more sensible course seems to be to confine one's attention to those crucial issues that are of interest to every philosopher, but have been somewhat neglected in these islands. Very few British writers have written intelligibly about intuition, about its function in religious experience, or about the chief modern theories of religion. Few have any knowledge of what Existentialism really is. Fewer still have any ideas regarding Personal Realism. It is with these topics alone that the reviewer proposes to deal.

What then is intuition? On this topic, the author is deplorably vague. He distinguishes between external and internal intuitions. He expresses the view that intuitive religious beliefs are rational interpretations of internal intuitions (pp. 50-56, 118-130). Such ill-defined generalities lead nowhere, and the author has no excuse for offering them, since he cannot but be familiar with the writings of Franz Grégoire on this very subject. According to Grégoire, *strict intuition* is the apprehension of particular reality without any intermediary. This occurs in pure sensation, in the awareness of pure affects, and, if Bergson is right, in the awareness of a common vital impulse. *Mediate intuition* is the apprehension of particular reality through the mediation of some strict intuition which is not recognised as such. This occurs in perception, and may perhaps also occur in trained introspection, and in some types of acquired religious experience. *Conjectural intuition* is the real or apparent apprehension of reality as a result of processes of inference of which one is not aware. This has so many different forms that it is hardly worth while trying to enumerate them. It occurs when one recognises one person by his family resemblance to another, and in those presentiments which geniuses have regarding general principles that are as yet undiscovered. This classification of intuitions, which is a needful preliminary to any further researches into the subject, excludes from their scope rational insight into general truths. That is no great loss, since this is not an ordinary sense of the term.

What part does intuition play in religious experience? Most people would say that one has a strict intuition of the limitless significance of one's own being. The Christian doctrine of the soul seems, in fact, to be an attempt to formulate this intuition in suitable imaginative terms. Indian mystics allege that it deepens as one passes from waking to dreaming, and from dreaming to deep sleep. Bergson interpreted it as a consciousness of a common vital impulse. Mystics like Scheler believe that this strict intuition forms the basis of a mediate intuition of God's existence—a perfectly sensible doctrine, provided one does not attempt to define the object too precisely. Most beliefs that pass for religious intuitions, however, belong to the third class, and owe their plausibility to the fact that they are connected with emotional intuitions

of the first class, although they are usually ideological distortions of them. This point is worth dwelling upon. When one reads in Genesis 2, 7, 'And the Lord God formed man out of the dust of the ground . . .', one instantly experiences an emotional shock. Either this revulsion must be repressed as an undesirable atavism, or else one must define one's objection to it by interpreting the experience more carefully. This might well lead one to formulate the intuition, 'Life is inseparable from moisture'. If one did so, one might easily be led on to observing the connection between the two, and gradually one's intuition would approximate to a physical perception. It seems likely that a mystic whose life is soundly rooted in reality has many such religious perceptions, and that the perception of the Holy, which Rudolf Otto mistakenly thought to be a crude datum, is in fact a habit-acquired perception.

What are the chief views that have been put forward to account for the religious consciousness? Here the author's treatment is much more satisfactory. He distinguishes such theories into three classes—Idealism, Intuitionism, and Realism. The attempt to reduce religion to a purely cognitive basis ends either in Hegelian Mentalism (pp. 34-35), or in an atheistic value philosophy, like that of Nicolai Hartmann, which recognises no form of cognitive religious experience but rational insight (pp. 18-23). Intuitionism, which is an attempt to reduce religious knowledge to a perceptual basis, presents a somewhat wider range of alternatives. Firstly, there is Kant's Moral Intuitionism, which is self-stultifying, because it denies the metaphysical validity of the very intuition upon which it rests (pp. 7-10). Another is Scheler's Phenomenological Intuitionism, which assigns to reason the function of interpreting and supplementing one's intuition of the absolute (pp. 10-18). Still another is Bergsonism, which treats this experience as an intuition of the vital impulse, and associates with it a further conjectural intuition of the evolutionary factor in experience (pp. 26-27). Lastly, and pre-eminently, there is Existentialism, of which this book gives a very full and fair account (pp. 31-103).

What is Existentialism? Historically speaking it is a philosophy of will that stems from that unhappy Danish genius, Soren Kirkegaard (pp. 31-45). Kirkegaard came to the conclusion that man ascended to a knowledge of the divine through a process of disillusionment. First came the aesthetic stage, then the speculative, then the ethical, and lastly the religious, which was a tragic conviction of sin, from which there was no way of deliverance except free obedience to the will of God as declared by the individual conscience in concrete conditions. One can hardly help agreeing with Father Ortega that such a moral theology offers little hope of salvation, although that does not imply that it is actually false. What is clear is that Kirkegaard had stumbled upon an all-important fact about genuine, first-order moral judgments, and that different philosophers have erected different philosophies upon this fact, which still retain enough community of doctrine to merit a common name.

The doctrines common to all forms of Existentialism may be grouped under four heads.

(1) *A Doctrine of Volition* (pp. 56-67, 130-138). Man's will is free, but he is confronted by alternative courses of action, between which he has no rational ground of choice. This situation produces an emotion of anguish, from which there is no deliverance except by what is, from the

strictly ethical point of view, an arbitrary choice. Commitment ends anguish, but what happens then is a matter of dispute among the different schools of Existentialism. According to the Nihilists, like Jaspers and Sartre, that is the limit of one's achievement—a deeper consciousness of one's limitations. According to the Constructivists, of whom Marcel is the chief, commitment leads to a fuller intuition of one's own nature, of the world in which one lives, and of the existence and nature of God. By such acts personality is formed.

(2) *A Doctrine of Value* (pp. 45-50). According to the doctrine of *Principia Ethica*, it is possible that only one thing should exist, and that that one thing should be evil. According to the Existentialists, this is impossible, because existence is itself valuable. One thing may be less valuable than another, but nothing is absolutely evil. This doctrine may not commend itself to clever verbal analysts, like Mr. Duncan Jones and Professor Stevenson, but it gives a note of realism to the writings of Sartre and Marcel, which must be welcome to a people who have suffered as deeply as the French.

(3) *A Doctrine of Knowledge* (pp. 50-56). All knowledge is intuitive in origin. Apart from strict intuitions, the only knowledge we possess is derived from acts of commitment. This applies as much to knowledge of one's own nature, as to knowledge of the external world. In these matters, Existentialism is very close to Pragmatism.

(4) *A Doctrine of Society* (pp. 67-76). Society does not result from a community of ideas, or a mutual contract, but from acts of commitment that recognise the existence of other subjects and of oneself as a subject in their eyes. Different schools have used this doctrine to justify the existence of different kinds of social organisation. Heidegger, for example, attempted to justify a Nordic racial state as the natural result of mutual acts of commitment on the part of people of that racial type. Others have used it to justify the existence of a totalitarian, visible Church. Both uses are defensible, if society is to mean something more than mere tribalism, but the history of Christianity suggests that there are special difficulties about acts of commitment involving formally-defined tenets, and the history of totalitarian states is even less inspiring. Perhaps it is only through long experience that mankind will learn which acts of mutual commitment are advantageous, and which are trivial or disastrous.

The weakest point in Existentialism is clearly its denial of universals and of discursive knowledge. A very clever constructive interpreter might perhaps be able to meet this difficulty by treating existential intuitions as the perceived convergencies of complementary thought-schemes—the sort of structures that Wittgenstein used to call 'hypotheses'. If, for example, one rushes for a train and finds it gone, one at least knows for certain that breakfast was late. If one had not committed oneself to the view that one could catch the train, one might never have known this fact for certain. Even so, relations of similarity and difference must be apprehended intuitively. The author does not attempt any such feat of interpretation. His criticisms are straightforward and annihilating, because he has a theory of his own to put forward.

Personal Realism is in essence the orthodoxy of Louvain (pp. 211-239). It accepts the view that the external world exists, and that we have genuine, though limited, knowledge of it in sense-perception. Though it explains the origin of these and other metaphysical beliefs by strict intuition, it

justifies them by experiences resulting from voluntarily-initiated actions. Blondel, for example, maintained the view that the idea of God was implicit in the idea of any finite thing, but that it was only when practical experience had demonstrated the validity of the belief in finite things that we could venture to affirm the existence of their ideal complement, God. If that thesis has not yet been put in absolutely convincing form, it at least presents a very attractive programme for a theistic argument. In general, we may say that Personal Realism admits the psychological reality of one's intuition of God, Freedom, and the Infinite Significance of the Soul; but it insists that they must be verified and interpreted by practical experience.

There is therefore some force in Father Ortegat's contention that Personal Realism has a place for both intellect and will, which neither Idealism nor Intuitionism alone can offer; though one cannot help wondering whether a clear statement of each of these different doctrines, not as philosophical caricatures, but as honest interpretations of experience, would not have resulted in complete unanimity. Personal Realism leads almost inevitably to the view that intelligent, practical love is the fullest expression of personality, and if one admits the theistic hypothesis, that would seem to entail the reasonableness of a Divine Society as a mediator of these values. If so, Father Ortegat is perhaps right in maintaining that those who have given their lives wholly to the furtherance of that society and to the exploration of those vital social truths have a special claim to the respect and loyalty of their fellow men (pp. 190-209, 239-246).

ARTHUR THOMSON.

The Sociology of Religion. By JOACHIM WACH. Kegan Paul, Trench, Trubner & Co. Ltd. 30s.

SEVERAL aspects of the sociology of religion intimately concern philosophers. The efficacy of religions in promoting communal *esprit de corps* and in bringing special communal groups into existence cannot be ignored by the social philosopher. Perhaps the duties of the priest-king, and the penalties imposed by society when he fails in them, illuminate the doctrine of sovereignty. In the field of human personality, Professor Wach (following Max Weber) discusses the sociological effect of religious *charisma*, and brings out the important distinction between the personal and institutional varieties of it. For the philosopher interested in language there are intriguing references to the languages reserved (as among the Trobrianders) for sacred occasions, though the reader is referred to Malinowski for details.

On all these points Professor Wach says something, but his book fails to satisfy. It attempts too much, and its aims appear to be confused. The result is an amalgam of every kind of book which could have been given its title. The expert is burdened with elementary details and explanations, the non-specialist with reviews of the present state of the subject and with frequent terminological discussions. The author seems at times to believe that propounding a terminology, if done in sufficient detail, is a substitute for analysing the facts. In some of the descriptive passages the book comes to life, but it is marred by a turgid style, and too often by involved argument. Moreover, the attitude of learned

common sense degenerates too readily into an elaborate triteness where all general principles disappear under their *caveats*. An immense array of footnotes makes up a bibliography full enough to be of great value; but both it, and the text, would have benefited from mutual separation.

C. J. HOLLOWAY.

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ERRATUM.

Mr. Martin Johnson's book *Science and the Meanings of Truth*, reviewed in the January number of MIND was erroneously described as published by Kegan Paul. It was actually published by Faber & Faber.

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